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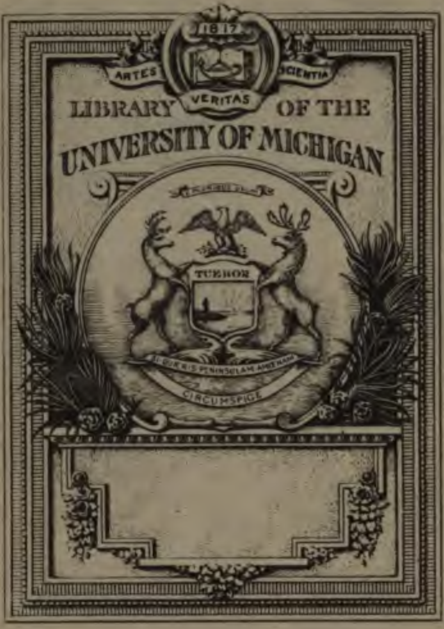

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THE

NEW-ENGLAND JOURNAL

OF

MEDICINE AND SURGERY,

AND

Collateral Branches of Science.

CONDUCTED BY A NUMBER OF PHYSICIANS.

Vol. VI.

> Homo naturæ minister et interpres tantum facit et intelligit, quantum de naturæ ordine, re vel mente, observaverit; nec amplius scit aut potest.
>
> FRANCIS BACON.

NEW SERIES, VOL. I.

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1817.

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Address delivered on the Induction of John Gorham, M. D. as Erving Professor of Chemistry in Harvard University, December 1816.

[Communicated for the New-England Journal of Medicine, &c.]

THE tendency of matter is to rest. All the motions of the particles of bodies with which we are conversant, may be regarded as so many efforts to equalize the forces which affect them; to constitute unchangeable compounds, and to reduce our globe to a state of eternal solitude, silence, and repose. But long experience has proved, that this equilibrium has not yet been produced, nor from the present order of things is it likely to happen. The general powers of nature, although they are never intermitting, are yet often unequal in their action; and it is to the varied operation of heat, of light, and of electricity, that we are to look for the cause of the complex and apparently irregular motions, which affect the different elements of matter, and determine the composition of the bodies observed on the surface of the earth. The atmosphere which surrounds, and the masses of water which float on the globe, are influenced by these powerful principles; they become secondary agents in the revolutions of the forms of matter, and produce the most general and important effects, either by their momentum, or the attractions of their component parts. Hence it is, that the surface, which is constantly exposed to the action of these powers, exhibits an endless succession of motions and of changes. The forms of matter are continually fluctuating, and the substance which, at one period, is solid, may at another exist as a liquid, or be resolved into air. The properties of bodies are equally altered with their forms; their particles alternately unite and recede from each other; if compound, they are decomposed and their elements are made to assume new arrangements; if simple, they combine with other matter, and, in that state, are no longer to be identified. Every thing is in motion and activity; and, although the series of changes is sometimes slowly progressive, and, to a superficial, observer, not always perceptible, it is, nevertheless, constant and uniform. It is the duty of the chemist to observe these changes and investigate their causes; to determine the nature of bodies, and reduce them to their elements; to ascertain their mutual actions and relations; and to apply the knowledge, acquired by observation and experiment, to the improvement of the arts which supply the wants, multiply the comforts, or administer to the luxuries of social life. Chemistry, therefore, is a science as important as it is extensive.

Like the other branches of physicks, which are founded on observation and experiment, the origin of chemistry is too remote to be distinctly traced; and it is obvious, that facts, which daily come within the cognizance of the senses, and which have an important influence on the existence, or the relations of life, must have early attracted the attention, although they did not lead to the investigation of their causes or connexions. But the physical truths which experience must have forced upon the ancients were unconnected, and we are not to look to them for the generalizations, which constitute a science. The early history of chemistry, therefore, presents nothing to the mind but the simple truth, that its progress must have been exceedingly gradual among a people, who, although remarkable for the powers of intellect and the refinement of taste, yet failed in their philosophy, because "they reasoned more upon an imaginary system of nature, than upon the visible and tangible universe."* It would be a task, as irksome to me as uninteresting to you, to trace the progress of individual discovery and the gradual development of facts, from which the theories that now constitute the science of chemistry are deduced. A much more pleasing and appropriate subject may be found in the revolutions, which these theories themselves have successively experienced, as observations have been extended, experiments have been multiplied, and new facts have been accumulated. I propose, therefore, to confine myself to a cursory detail of the state of the science, as it has existed at different periods, and to conclude with a view of the philosophy of the present age.

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* Sir Humphrey Davy.

The first opinions which may be regarded as an approximation to chemical hypothesis, were those of the alchemists, an extraordinary body of men, who flourished between the 11th and 16th centuries. During the period of the crusades, the warriors of the west, whose zeal was excited, and whose wealth was bestowed for the glory of the cross, gradually lost, with the success of their arms, the enthusiasm and spirit of chivalry which led them to Palestine. They saw themselves deprived of their possessions, reduced to poverty, and incapable of supporting their rank in society, and they willingly turned from the unprofitable fame of champions of religion, to the hopes held out to them by the impostors of the east, of the power of transmuting the metals into gold, and of obtaining immortality upon earth. They transferred the delusion from Asia to Europe, and thus gave rise to the study and pursuits of alchemy. The cultivators of this art may be considered as the forerunners of modern philosophy: with the spirit of physical research and experiment, which has distinguished their scucessors, they combined all the mysticism of the Cabalist and the absurdity of the Dialectic school.

About this period the mind, under the guidance of the scholastics, had taken a wrong direction; and no system ever invented, could be more unfavourable than this to the pursuit of polite literature, or to the progress of demonstrative science. The lives of the learned men of the time were devoted to the art of disputation. They confounded each other, and confused themselves, by subtle and ingenious distinctions in terms; by a refined and speculative system of words; by recondite and sophistical modes of reasoning; and by their attempts to obscure the boundaries of science, and to unite physical with metaphysical subjects. Great minds were sacrificed in the pursuit of these illusions. Had such men as Thomas Aquinas, William de Campeaux, Albert of Cologne, and the unfortunate Abelard, who lives only in the numbers of the poet, investigated material nature with the same ardour, with which they entered the fields of controversy, and the same sagacity which they displayed in fixing the signification of a word, their labours would have been crowned with brilliant success, and their names with unfading glory. The alchemists explored a new and more fruitful subject. Leaving the study of logic, and the cultivation of the dialectic philosophy to those who aspired to literary fame, or ecclesiastical distinction, they commenced the practical examination of nature. But destitute of instruments, unused to methodical arrangement, and unacquainted with the processes of investigation which are required by the severer sciences; guided by enthusiasm and stimulated by avarice, the objects of their pursuit were as absurd, as the means they employed to obtain them were extravagant. They believed that the basis of all the metals is gold, and this class of bodies was tortured, with the most persevering industry, to discover the art of removing the alloying substance, and of acquiring unbounded riches. They thought that it was in the power of human art to compound a liquid, which should renovate the frame, when impaired by time, or exhausted by disease, and be able to restore to the rigid and unyielding fibre of age, the bloom and elasticity of youth. They imagined, also, that nature had formed a substance of such astonishing powers, that no other body could resist its action; and the alkahest, or universal solvent, was looked for in the products of the crucible and the alembic, with the same anxiety as was exhibited for the elixir of life, and the philosopher's stone. On the subject of general physicks, they were less absurd. They rejected the four elements of the ancients, and supposed all nature to be composed of the three simple principles, salts, sulphur, and mercury. Such were the speculations and the pursuits of the alchemists. A feeble ray of light from the source of science would occasionally penetrate through the gloom in which they were enveloped, and conduct them to some important fact. The most distinguished among the alchemists was Paracelsus. This man possessed a bold and original mind, which, conscious of its powers, but just released from the trammels of the schools and the weight of ancient opinion, knew not how to direct its views, or limit its operations. He was the first public lecturer on chemistry in Europe; and this station, together with the reputation derived from the successful application of chemical medicines to the cure of diseases, afforded him the opportunity of attacking and of prostrating the dogmatic school of Galen. In his character were combined the extremes of arrogance and meanness, of the freedom of truth with the boldness of pretension, of enthusiasm of mind with great profligacy of manners. Yet he was, probably, an eloquent man. In his wanderings he was followed by a crowd of admirers; his opinions, which were respected as the decisions of an oracle, were embraced with zeal, and disseminated throughout Europe; and although, notwithstanding he declared himself the possessor of the elixir of life, he expired, exhausted by his vices, at the early age of forty-two, we have sufficient evidence of his celebrity in the epitaph engraved on his tomb-stone, at Saltzburg.

> Divinus talis est Paracelsus in arte monarcha,
> Similem cui secula nulla tulere virum.

In reflecting on the influence which the pursuit of alchemy has had on physical science, it must be acknowledged, that it has been greater than would, at first view, have been imagined. It diverted the mind from the study of words, to the examination of things; and, as at that period, experiments on the nature and relations of bodies were altogether novel, it can hardly be supposed, that however absurd and impracticable the specific object of the alchemist might have been, he should not have arrived at new results, unexpected discoveries, and useful facts. We accordingly find, that to them belongs the merit of having demonstrated the properties of quicksilver and of antimony; of the three most important acids, and of ether; and of having introduced into medicine a variety of active preparations. When the knowledge of the nature of bodies became more accurate and extensive, and, more especially, when experience had demonstrated, that the pursuit of the philosopher's stone ended in poverty and disgrace, the belief in its reality gradually lessened, and the delusion terminated with the life of Libavius.

The theory, which, after some time, succeeded the speculation of the alchemists, originated in Germany; and that country, which has been recently stigmatized as the school of pedants, the abode of dulness, and the asylum of the enthusiast, swayed, for nearly a century, the opinions of the chemists of all Europe. Beccher, who was born at Spires, in 1645, produced his two remarkable works, Physica Subterranea and Œdipus Chemicus, in which he advanced the bold and magnificent theory, that there exists in nature but one simple elementary principle, to which he gave the name of earth. This was divided into three species: the vitrifiable, the metallic, and the inflammable. Few facts, however, were, at that period, collected, to substantiate the hypothesis; and it was regarded, rather as the work of a brilliant imagination, than as a legitimate theory, founded on facts and susceptible of demonstration. It was reserved for Ernest Stahl, of Vienna, to build a simple, consistent, and beautiful system, on the original idea of his master.

But, previous to this period, a revolution, the most auspicious to the cause of physical science, had been completed. The mind, which had been so long depressed by ignorance and superstition, suddenly resumed the exercise of its powers. The weight was removed, and it rose by its own elasticity. Lord Bacon had already pointed out the only mode by which the faculties are to be assisted in the study of nature; and the greatest praise is undoubtedly due to him for diffusing, by his writings, the knowledge of this method, and the use of the means by which man is to attain to physical truths. But it must be remarked, that he wrote at a time when the mind was already prepared for his plan; and that a few works connected with science had already appeared, in which, although nothing is expressly said on the advantages arising from the method of induction, the subjects are treated of in the same philosophical spirit. The work of George Agricola of Germany, de re metallica, had been published eighteen years, when the British philosopher first saw the light. Learned societies were founded; and zeal in the cause, and success in the pursuit of truth, were promoted by union of efforts. The Academy del Cimento at Florence, published a series of useful facts and valuable experiments. The Royal Society of Britain could boast of Slare, of Boyle, and of Hooke; and the French Academy of Homberg, Geoffroy, and the two Lemerys. The most important instruments of physical research were rapidly succeeding each other, and the gravity of the air, which had been demonstrated by Gallileo, and measured by the Barometer of Toricelli and Paschal, was merely an introduction to the knowledge of its other mechanical properties, obtained by the air-pump of Otto Guerick and Sir Robert Boyle. The Florentine Academicians and Mr. Boyle gave to philosophy the thermometer, and many of the numerous relations of heat were soon unfolded. Newton first raised chemistry to the dignity of a science, by applying to the particles of matter his general principle of attraction; and not long after the knowledge of the relative forces of attraction in different bodies was rendered definite by the tables of Geoffroy. At this period, the attention of philosophical minds was withdrawn from the investigation of the elements and mutual actions of bodies, and chemical research was retarded. The sublime views which the philosophy of Newton unfolded, of the motions and relations of the celestial system, and the profound demonstrations which it exhibited in the higher mathematicks, not only allured the attention, but excited the enthusiasm of every man of science. Astronomy was cultivated with all the ardour which had before been devoted to chemistry, and men turned, with something like contempt, from the examination of the matter on which they trod, when the sublime machinery of the universe was opened to their view. But as the system of this great philosopher became familiar, and mechanical philosophy assumed the form of a science, the actions of the particles of bodies again attracted notice. Practical chemistry was pursued with the zeal, which arose from the conviction of its usefulness, and with the pleasure which is derived from gratified, but unextinguishable curiosity. It was at this period, when the nature and properties of a multitude of substances were accurately known; when the processes of the chemist were refined by a correct judgment, and conducted with much manual dexterity, that Stahl produced a theory which excited the admiration and commanded the assent of all Europe.

The process of inflammation is so intimately connected with chemistry, that a theory of combustion was, for a long time, supposed to constitute the science itself. It was thus, in a great measure, regarded by Stahl. He inferred, that there is a principle existing in all combustible bodies, which may be considered as the cause of their inflammability. This principle, which was supposed to be more attenuated than the earth of Beccher, or the ethereal fluid of Newton, and analogous to light, heat, or pure fire, he called phlogiston. He assumed, that while bodies are burning, the phlogiston is in the act of disengagement; that when it is completely evolved, the body is burnt, and, in that state, is incapable of going through the same process a second time; that phlogiston may be slowly disengaged without the usual phenomena of combustion, by which the theory was extended to other chemical operations; that the light and heat observed in active inflammation, are the results merely of the violent motions of the particles; and, lastly, that phlogiston may be silently transferred from one body to another, the former losing, and the latter acquiring the property of combustibility. This simple and elegant theory was supported by so many well chosen experiments, it was arranged with such consummate skill, and exhibited throughout such proofs of a cautious and philosophical spirit, that it received unlimited assent, and was publickly taught with the same conviction of its truth, as was accorded to the demonstrations of Sir Isaac Newton.

But the period of rigid chemistry was approaching, when philosophical minds were to be satisfied only by demonstrations. The existence of phlogiston was hypothetical, and the belief in its reality was continued, rather from the ease with which it enabled chemists to explain the phenomena of the science, than from any precise knowledge of its nature, or any actual experience of its properties. This difficulty, together with the important fact announced by Rey and others, but overlooked by Stahl, that bodies increase in weight while burning, gradually weakened the enthusiasm in its favour. It was necessary either to reject it altogether, or so to modify it, that it should be applicable to the new facts. Fortunately for its reputation, the discovery of the elastic fluids, and particularly the demonstration of the properties of hydrogen, furnished the chemists with an agent, the nature and properties of which had been accurately unfolded, and which might be substituted for the imaginary phlogiston of Stahl. This principle then was supposed to be essential to the combustibility of bodies, and when it was disengaged in the act of burning, its place was supplied by oxygen. The augmentation of weight was satisfactorily accounted for, and the theory was again completed. It only remained to demonstrate the existence of hydrogèn in all combustible bodies; but this was beyond the art of the chemist. It was in vain, however, that such men as Cavendish, Kirwan, and Priestley, laboured to establish the modified theory of Stahl, on a permanent and unchangeable basis. The tide of popular opinion had already turned in fávour of the French system. It gradually receded from view, and was suffered to repose, at least for a time, with the ashes of the celebrated and unfortunate Priestley.

The system which was erected on the ruins of that of Stahl, originated in France, and its author was Lavoisier. No man perhaps was better calculated to give currency to new opinions. Enjoying all the influence which political distinction, a splendid fortune, and the reputation of uncommon talents can bestow; trained for the severer studies, by an early and judicious education; well acquainted with the science of the age, and already distinguished as a zealous cultivator of philosophy, he carried into chemistry all the precision of the exact sciences, and reasoned on their phenomena with the caution of the geometrician. He knew how to avail himself of the labours of others, and his doctrines were founded on the discoveries of Black, Cavendish, Priestley, and Scheele. The basis of his theory is founded on the phenomena which oxygen exhibits in its combinations with other bodies; and the enunciation of the doctrine itself is unfolded in the simple propositions.

That combustion will not go on without the presence of oxygen.

That in every case of combustion this air disappears, the burnt body increases in weight, and the weights thus lost by the one and gained by the other, are precisely equal; that combustion, therefore, is the act of union of the inflammable body with oxygen.

That oxygen is the principle of acidity.

That it may be transferred from one substance to another, without the usual phenomena of inflammation, and

That the heat and light, both of which are regarded by him as material, and which are disengaged during the combinations of this gas, are the results of the difference of capacity between oxygen air, and the product of its union.

Such was the simple, consistent, and beautiful system of Lavoisier; but notwithstanding the high reputation of its author, and its adaptation to the facts which were at that time accumulated, he could not, for a long time, boast of a single convert. It was at length embraced by the distinguished chemists of France; it gradually forced its way into Great Britain, and was finally adopted, although not without opposition, throughout Europe. This revolution, however, in the philosophy of chemistry was not yet completed. The language employed in this science was derived, either from the Arabic, or from the more fantastic and barbarous phraseology of the alchemists. The names imposed upon recently discovered bodies were perfectly arbitrary. The entire change, or at least the reform of nomenclature was universally acknowledged to be necessary, and the period chosen for this change was peculiarly favourable to its diffusion and adoption. The labour was undertaken by Lavoisier, and, in conjunction with Morveau, Fourcroy, and Berthollet, he published, in 1786, a new nomenclature founded on his own system, and deriving its radicals from the language of the Greeks. From this time, the reputation of French philosophy was identified with the success of the new system, and by methods, equaly judicious and ingenious, it gradually became the language of all the chemists of Europe.

This nomenclature is founded on the nature and the number of the elements of which bodies are composed, and it must be acknowledged, that it was simple and uniform, adapted with exquisite skill to the state of chemical knowledge at that period, and applied with the greatest ingenuity to the different forms of matter. Had chemistry been circumscribed, and its limits accurately known, it would have remained an everlasting monument of the science and sagacity of Lavoisier and his associates. But it was premature. In proportion as novel facts are developed, and as new elements are discovered; in proportion as the art of analysis is refined, and we approximate to the knowledge of the real nature of bodies, it is obvious, that a theoretical and composite nomenclature must be equally varied' to correspond with these changes. As chemistry is a science founded on experiment, no bounds can be set to its progress, nor, consequently, to the language which, like that of the French system, is intended to illustrate the nature and composition of bodies; and it must, therefore, sooner or later, lose, with the theory itself, the qualities which led to its adoption, simplicity, precision, and consistency. Such will be, nay, I may venture to say, has been the fate of the system of Lavoisier. Oxygen is not the only supporter of combustion; it is not the principle of acidity; it is not the source of the light, nor, in all cases, of the heat, evolved during its combinations, and it is hardly the rival of hydrogen, or the phlogiston of Kirwan and Priestley, in the number of its combinations, or the importance of its phenomena. It is already fallen, and the future chemist will regard it with the same mixed feeling of admiration and regret, with which the scholar contemplates the ruins of the Grecian temple over which time has passed his relentless hand, has withered its beauty, and levelled the harmonious structure with the dust.

To account for this rapid downfall of the French philosophy, it will be necessary to take a cursory view of the present state of chemical knowledge.

Since the commencement of the 19th century, more has been done to place the science on a permanent basis, than had been effected by all the labours of preceding chemists. The grand discovery of Volta, that Galvanism, by certain arrangements of different metals, may be accumulated to an almost indefinite extent, has given to the chemist an agent of great power in producing decompositions, and in arriving to the elements of bodies; and the application of voltaic electricity to different forms of matter, opened to Sir Humphrey Davy a glorious career, and crowned him with unfading reputation. He has not only discovered a number of important elements, and given probability to the hypothesis of an universal inflammable principle, but, what is of more consequence, he has nearly established the general truths, that the mutual attractions of the particles of different matter are proportional to their electrical energies; and that those which obviously have the greatest tendency to combine, are such as also exhibit the most opposite states of electricity. So great and decided is the number of facts to support this opinion, that Berzelius, the distinguished chemist of Sweden, has boldly published a system founded upon the electro-chemical energies of bodies. This may be considered as an important step toward the completion of the science; for, instead of the obscure and indefinite idea, of attraction, and of a power which is unseen and unknown, chemists will now recognise the agency of a principle, which may be accumulated or discharged at will, the properties of which may be accurately demonstrated, and which, when it acts on masses, instead of the atoms of matter, is known to produce some of the most sublime and magnificent phenomena of nature.

It is not a little remarkable, that chemistry, although it is founded on experiment, should not, until the present moment, have assumed the form of an exact science. The play of attractions has been supposed to vary so much by the circumstances in which bodies are placed, and so much uncertainty has appeared to exist, relative, either to the general results, or to the proportions in which they unite, that it has been said, experiment alone can enable us to acquire any positive knowledge on the subject. This uncertainty will no longer exist, and by far the greatest improvement which has yet been made in this science, is the discovery of the general truth, that bodies combine in definite proportions. The atomick theory is the production of the present age; for although some of the facts were unfolded by Dr. Higgins, the merit of the generalization belongs to Mr. Dalton of Manchester. It infers, that bodies are composed of atoms, which are not mathematical, but physical points, and are, consequently, possessed of extension and gravity; that these atoms unite with each other in one or more proportions in simple ratios, and, as has been proved by experiment, that the higher proportions are multiples of the first. By this simple law, chemistry is at once made to take rank with the physical sciences which are founded on the mathematicks. The relative weights of the atoms of different bodies may be estimated, the proportions in which they combine may be accurately defined, and calculation may thus be made to correct, or supply the deficiencies of experiment. The theory of volumes which has arisen from the discoveries of Dr. Higgins, and of Gay Lussac, and been adopted by Sir H. Davy, is equally founded on the laws of definite proportions, and applicable to all the phenomena of true chemical combination.

Another subject which appears to be connected with the electrical energies of bodies is crystallization. The fact, that the immense variety of crystals which are disseminated throughout nature, may be reduced, by mechanical division, to a few primitive and elementary forms, which was suspected by Bergman and De Lisle, has been demonstrated by Haüy; and the crystallographical researches of this philosopher have unfolded to us one of the most beautiful and recondite processes of nature. The cause which produces these crystals, is without doubt, the power which gives polarity to their atoms. It is thus connected, on the one hand, with mechanical philosophy, and on the other, with chemical science. The laws of crystallization, of definite proportions and of the electrical energies of bodies, says Sir H. Davy, seem to be intimately related, and the complete illustration of their connexion will probably constitute the mature age of chemistry.

Chemists have penetrated into all the departments of nature, and no subject has been investigated with more zeal, and I may add, with less success, than the chemistry of organized beings. The various actions which take place among the elements of vegetables and of animals, the numerous changes which they undergo, the variety, and peculiarity of their products, and their nice adaptation to the purposes for which they are supplied, could not fail to attract the attention, and engage the researches of men, who were already familiar with the properties and composition of common matter. The right of explaining the functions of the living body was assumed by the chemists; and the physiologist was no sooner relieved from the absurdities of the mechanical doctrines, than he was bewildered in the mysteries of chemical affinities. The component parts of the human body have been analyzed with the most scrupulous exactness. Man has been reduced to a few simple principles, or elements, and the changes which take place during life, have been explained with the same ease as those, which disorganize the body after death. Nay, the zeal of the chemists has carried them so far, that they have attempted to compose some of the proximate principles of animals by mixture, or by the powers of voltaic electricity. There are some, in this country, who have thought, and have even published the brilliant idea, that the chemist may soon be able to crystalize a man, and animate his crystal by the agency of electric matter. It would be useless to attempt to refute an opinion so absurd and so impious. The laws of animated nature do not appear to be the same with those of chemistry, and the impotence of the chemist, in this department, is clearly evinced by the single fact, that, notwithstanding all the analyzes which have been made, he has not yet succeeded in producing a single compound, similar in every respect to any of those which are elaborated in the living system. The powers which regulate living matter, seem to have no other connexion with those by which inanimate nature is governed, than in operating on the same elements. These materials are combined in different ways, and by methods which the laws of chemistry cannot control, and in which their part, if any can be allowed them, is feeble and subordinate. The actions and combinations which produce digestion, assimilation, secretion and respiration, are vital. They no more belong to chemistry, than the properties of common matter make a part of vitality, and vital affinity is probably as distinct from chemical attraction, as sensibility and irritability are from extension and gravity. The departments of the chemist and of the physiologist are well defined. Living matter is the property of the latter, inert inanimate nature of the former.

Such is the present state of chemistry. It is distinguished from all other periods of the science by the probable unalterability of its philosophy. The theories of definite propositions, of the electrical energies of the particles of bodies, and of the operation of the powers, concerned in the production of chemical phenomena, constitute what may strictly be called the science; and, however individual facts may be multiplied, or its processes refined, its laws, founded on experiment and supported by the mathematicks, must remain unchangeable. Combustion which, for so long a period, was considered as the science itself, now takes a subordinate rank. Hydrogen has been discovered to constitute a part of all classes of bodies, except the metals, and should they be proved to contain it, the doctrine of phlogiston must be renewed in a demonstrable and permanent form. Oxygen is not the principle of acidity, for it forms an essential element of bodies, which are alkaline; nor is it the only supporter of combustion, for this property belongs also, to two other bodies, chlorine and iodine.

Were I permitted to exceed the usual bounds of a discourse on these occasions, I should dilate, with the greatest satisfaction, on the intimate relation which exists between chemistry and the arts; and on the important advantages which have resulted from the application of philosophical principles to the improvement of the most useful and familiar processes. Nor should I forget to impress on the minds of those who are, or may be engaged in the study of medicine, that the road to fame, in this profession, leads through the fields of natural history, of chemistry, and of mechanical philosophy; and that the reputation of Boerrhaave and Cullen, of Fothergill and Fordyce, of Rush and, may I add, of Warren, was founded on the knowledge of the collateral branches of medicine. But I must forbear.

Chemistry, then, is a science of experiment, and its object is to discover the elements and the relations of bodies. When it is recollected how many distinguished men are now engaged in these pursuits; with what zeal they advance through new, and hitherto untrodden, paths; with what delicacy and precision they operate, and with what discoveries their labours have been already rewarded, it must be acknowledged that, although rapidly progressive, no limits can be assigned to this science. In proportion as the instruments, with which they operate, are increased in power or improved in construction; and in proportion as the habits of accurate observation and of cautious reasoning are cultivated, new elements will be discovered, or those, which were once regarded as simple, will be proved to be compound. The number of classes of simple substances will be reduced; it will be found that the great Author of nature operates with fewer materials than has been supposed; and, possibly, the sublime idea of the ancient philosophers may be verified, that there exists but one simple primitive form of matter, the element of all nature. Chemical investigation has already approximated to the conclusion, that matter may be divided into the two classes of oxygen and the metals; and should the opinion, which is now held by many, respecting the constitution of some of the elastick fluids, be proved, as it probably will, to be correct, all the powers engaged in the production of chemical phenomena, and all the elements of bodies may be reduced to radiant matter, to the supporters of combustion, to the acidifiable bases, and to metallick bodies. An arrangement like this, of the substances which constitute, and of the powers which act on the globe, cannot fail to strike the most superficial observer with admriation and awe, and it leads us at once to the conclusion of Sir H. Davy, that the more the phenomena of the universe are studied, the more distinct their connexion appears; the more simple their causes, the more magnificent their design, the more wonderful the wisdom and power of their author.

A distressing case of Tic Douloureux, successfully treated by a surgical operation. By Dr. JEREMY STIMPSON, *fellow of the Massachusetts Medical Society.*

[Communicated for the New-England Journal of Medicine, &c.]

ON the first of April, 1816, I was called to visit Philip Cobbet, a man of about seventy-one years old, who gave me the following account of his case:

"In the year 1790, I had an exquisitely painful sensation in the right cheek, that came and went like a flash of lightning. About a year after, I had several fits of the same kind, which distressed and alarmed me so much, that I applied to old Dr. Sprague of Dedham; who gave me hemlock pills, which I took for several weeks and got relieved from the pains, and continued free from them, for six years. At the end of that time, I was again attacked with great violence, and have not since been free from the pains, for any long time, although I have taken great quantities of hemlock. I have, also, been once salivated by mercury, and have taken many other medicines without the least mitigation of my sufferings. For the last six weeks, I have experienced the most dreadful agony; for during this time, the fits have come on usually, as often as once in five minutes, by night and day, and I have not been free from them, in that time, for a longer interval than half an hour in the day, nor even in the night. The slightest touch of the cheek, and even speaking and eating bring them on with such violence, as almost deprives me of my senses, and what increases my distress, is, that no physician has known the nature of my complaint, so that some of the neighbours consider it, as in a great measure, imaginary."

After hearing this account, I immediately informed him, that his complaint was the tic douloureux, and advised him to undergo an operation for dividing the diseased nerves, as affording the only hope for relief. To this, he most readily assented. Dr. Warren, of Boston, was then sent for, to perform the operation, and we visited the patient together, on the 18th of April. We found him on the bed, feeble, emaciated with pain and watchfulness, and exhibiting the most deplorable appearance of misery. Before we had been in the room many minutes, he cried out, and with one hand immediately applied a folded cloth with violence to the cheek and pressed the other hand on the forehead; for, as he informed us, when he could strike the head soon enough, he succeeded in lessening the acuteness of the pain. While preparations were made for the operation, the pains did not appear to be prevented by the apprehension of it; but they occurred, as usual, once in four or five minutes, with a violence which surprised and shocked us. The duration of the pains did not exceed half a minute, and they departed as suddenly as they came, leaving him at ease in the intervals.

It appeared, that the original seat of the disease was in the right cheek, about the infra-orbitary foramen and following the branches of the infra-orbitary nerve. But now, the disease had reached the forehead and become even more severe in that, than in the part first affected. The pain extended even into the ear, so that we had fears that the facial nerve might have fallen into the same state, as the other nerves; for, in truth, the whole side of the head was so violently affected, from the angle of the lower jaw to the crown of the head, that the patient could hardly point out any one part, as the seat of greater pain than others.

Dr. Warren determined to cut off the infra-orbitary nerve, and, also, the supra-orbitary; and if this should be followed by a mitigation of the disease, without a perfect cure; to divide the facial nerve at a subsequent time. On placing the hand upon the patient's face, previous to commencing the operation, so severe a paroxism was brought on, as made it necessary to wait; and each time that the same circumstance occurred, the pain was produced with such violence, that the patient could not control himself. An incision an inch and half long was made through the integuments, about half of an inch below the orbit, and then the instrument was carefully drawn across the bone, so that no portion of the nerve might escape division. Immediately after, another incision was made above the orbit, something longer than the first, and quite down to the bone. On the division of the inferiour nerve, he exclaimed violently, "take care, you are knocking my teeth out."—The wounds bled freely. After the bleeding had subsided, lint was carefully pressed down to the bone and supported by additional lint and compresses, so as to be well interposed between the divided nerves, and I was desired to repeat this application for some time. The patient experienced some degree of relief immediately after the operation; but the change was not very remarkable, until the fourth day, when the paroxisms became less frequent, and by the seventh day after the operation, I had the satisfaction to find, that the pains had entirely left him. Notwithstanding this, I kept the wounds open for about five weeks.

Sept. 12.—The patient has had no return of his complaint, although it is almost five months since the operation. When he gets a cold, he has a sensation about the ear, in a small degree resembling his former disease; but this is so trifling as not to prevent his usual labours; nor has he been confined to the house a day, since the week succeeding the operation; although the weather has been peculiarly cold and changeable, such as formerly caused the most severe paroxisms of his complaint.

Dedham, Sept. 12, 1816.

On the effects of spurred rye, in promoting parturition.

[To the Editors of the New-England Journal of Medicine, &c.]

Gentlemen,

I WAS among the first practitioners in this quarter of the country, who employed the ergotted rye to promote the efforts of parturition, and I have continued to employ it, for several years, more or less frequently. I have also inquired among my fellow-practitioners in obstetrics, respecting their experience in the use of this article, and have compared their observations with my own. Nor have I omitted to read the publications relating to the subject, so far as they have come within my knowledge. I perused, with great pleasure, the learned discourse delivered before our society,* on this topick. The result in my mind, has been, that the powers of this substance to increase the force of the pains of labour, and thereby to hasten the birth, in certain cases, are indisputable. These powers are, indeed, wonderful, whether you consider the certainty with which their effects are produced, the rapidity with which they ensue after administering the medicine, or the actual amount of those effects. The uterine contractions from being rare, feeble and short, oftentimes become extremely powerful, of long duration, and almost constant; so that, in certain cases, the birth is effected in half an hour after the article is given.

The effects of the ergot are so great, that it is probable, that most gentlemen, who use it, have done some harm in their early trials, as I did, by giving it where the soft parts had not become sufficient relaxed. But this is a fault which is corrected by a little experience. When employed in proper cases, it certainly shortens a tedious labour, and thus benefits the patient. If the pains produced by it are violent, they are few in number. Meanwhile, it certainly is of great advantage to the physician, who would otherwise be kept for a long time in attendance on a case where nothing but effort is wanting for its termination.—Unless, then, some subsequent evil occurs, either to the mother or to the child, I think the ergot of rye should be added to the list of most valuable articles in our materia medica. But, unfortunately, some subsequent evil has been noticed, not to the mother, but to the child, and it is on this point, that I wish to offer my remarks.

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* By Oliver Prescott, M. D. Counsellor of the Massachusetts Medical Society.

I believe that the first intimation of any evil to the child, was given in one of the early numbers of your Journal. Dr. Prescott was not quite satisfied that there was any ground for the fears expressed; but I own that his statement did not entirely satisfy my mind. The piece was anonymous, and I suppose the author had the same objections to giving his name, which I now have; for a man does not want to publish to his neighbours, that he has been injuring them, although unintentionally. But as the piece was anonymous, I should not have noticed it much, if it had not made me recollect that the proportion of dead-born children, under my care, had been greater than usual, since I had used the ergot. I believe that the child was dead-born in the fourth case, in which I used it, although it was a fine healthy looking boy. Recollecting this, and two or three other cases of the same kind, and finding, on inquiry, that some of my friends in the profession had not been more fortunate, I began to think that your writer might be correct. I, therefore, resolved to be more careful, in future, and especially, not to give the ergot, unless every thing seemed ready, so as that I might be sure of no obstacle to make resistance. Likewise, I determined to give small doses, so as not to bring on those violent steady pains which we all have observed. In this way, I certainly got along better. It is certain, that my fears prevented my using the medicine very often. I will not conceal that I have lost one child since I meant to be very careful; but the dose was perhaps greater than I had intended, and there was less relaxation than I had thought for. But of late, I have been brought to consider the subject more seriously; for though I have not lost the children, yet in two instances, I think, I have come very nigh losing them. In two instances, as proper for the use of this medicine as any which ever occur, the children would scarcely breathe at first, they were almost black, and I could not recover them for a long time. The medicine was given in very small quantity, less than eight grains in each case, yet the effects were considerably powerful. In the last of these cases, the child bled at the nose, mouth, and eyes, and the respiration, long after it began, was quite sterterous. I feel satisfied, that the child would not have recovered, if I had not let blood at the navel-string. After all, I felt anxious for some days, lest blood should have been discharged within the cranium, as well as from the external passages about the head.

And now, gentlemen, I can no longer entertain a shadow of doubt, that the life of the child is endangered by this medicine. I could not help considering, that I should never be willing to have it administered to my wife, or to any of my relations. I therefore think the further use of it unjustifiable, while I entertain this opinion of it, unless it be in a case where the mother or the child is already in danger, and this may promise to save them. Accordingly, I have made up my mind to discard the use of this article. But this did not seem to be enough, and I have therefore thought proper to offer a publick warning to others, so as to prevent all the evil I can. If you do not think my remarks impertinent, you will be pleased to give publication to this note, which is longer than I meant it should be ; and oblige your constant reader, and a

FELLOW OF THE MASSACHUSETTS
MEDICAL SOCIETY.

——— Oct. 1816.

Chemical examination of the Water of the Congress, Spring Saratoga.

[Communicated for the New-England Journal of Medicine, &c.]

AS it is the professed object of this Society, to promote the knowledge of all natural productions, I flatter myself that the following observations on a *Mineral Water*, will not be considered foreign to its purpose. I was supplied with this water through the politeness of Thomas Foster, M. D. of Cambridge, who brought it from the spring. It was preserved in glass bottles, well corked, and the corks secured by wires.

The water of this spring sparkles when poured from one vessel to another ; is clear and transparent ; to the taste it is saline, with some degree of bitterness ; its sensible operation on the system is that of a diuretick and purgative ; the specific gravity of it is 1003.

The following phenomena were exhibited, on the application of the usual reagents.

1. Tincture of litmus was changed to a red.
2. Tincture of Brazil wood became red.
3. Nitrate of silver produces a copious precipitate.
4. Lime water occasions a copious precipitate, which is soluble in a few drops of muriatic acid.
5. Water of ammonia produces a copious precipitate, soluble in an acid.
6. Oxalate of ammonia produces a large precipitate.
7. Acetate of Lead throws down a precipitate, soluble in nitric acid.

8. Muriate of Barytes, and a solution of pure barytes occasion a precipitate, wholly soluble by the addition of a few drops of nitric, or muriatic acid.
9. Tincture of turmeric assumed a yellowish brown colour, but remained yellow and transparent in distilled water.
10. Carbonate of potash occasions a precipitate, soluble in acids.
11. Carbonate of ammonia produces no change, but on the application of Phosphate of Soda, in the method pointed out by Dr. Wollaston, an immediate precipitate appeared.
12. Phosphate of Soda produces a precipitate, without the previous addition of carbonate of ammonia; this precipitate, and that occasioned by carbonate of ammonia, and phophate of soda, (11.) are soluble in nitric acid, and also the precipitate which appears when phosphate of soda is added to lime water; hence the application of carbonate of ammonia and phosphate of soda, for the purpose of detecting magnesia, will be fallacious, if lime, either pure, or dissolved by carbonic acid, be present.
13. Muriate of platina produced no change.
14. There was no indication of iron by succinates, prussiate of potash, or tinc. galls.

In a quantity of the water which had been briskly boiled a few minutes, and during which an abundant precipitate appeared, tr. of litmus suffered no change; solutions of nitrate of silver, pure barytes, muriate of barytes, acetate of lead, lime water, and tr. of turmeric produced the same changes as in water which had not been boiled; but aqua ammoniæ, oxalate of ammonia, carbonate of potash, phosphate of soda and muriate of platina produced no effect.

The analysis of the water was attempted in the following manner:

A. One pint of water, carefully evaporated in a glass bason, dried at 150° of Faht, and weighed, while warm, afforded 78 grains of solid matter, which was white, and not deliquescent when exposed to the air, but a portion of the salt, obtained from a quantity of the water which was much concentrated for this purpose, and suffered to become dry spontaneously, evidently effloresced, when exposed to the warm, dry air of a room.

B. The solid mass was digested in two ounces of distilled water, thrown on a filter and thoroughly washed; the insoluble portion which was white, granular, earthy, and tasteless, was set aside for further examination.

C. The solution B. was evaporated to dryness in a glass bason; when much concentrated, a pellicle appeared on the surface, and on cooling, afforded a large quantity of muriate of soda; it was dried at 150° of Faht, and weighed, while warm, 56.5 grains.

D. The solid matter C. was muriate and carbonate of soda; to separate them, the carbonate was converted into an acetate, by adding to the dry mass, distilled vinegar, which produced effervescence ; it was then evaporated to dryness, and over it was poured a cubic inch of alcohol of the spec. grav. of, 815; it was suffered to stand, with frequent agitation, twenty-four hours; it was then thrown on a filter, and washed in repeated small portions of alcohol of the same specific gravity ; the matter remaining after the action of the alcohol, when dried, weighed 52.5 grains, and was muriate of soda. This method of separating salts of soda, soluble in alcohol, is the one which is generally used in the analysis of waters, and is liable to the objections, 1st, that a small quantity of the muriate may be dissolved in the alcohol, the action of which is probably increased by holding the other salt in solution; and 2d, that a portion of the salt intended to be separated by the alcohol, may adhere to the muriate, and thus escape its action; thus, notwithstanding the caution used in the application of the alcohol, a small quantity of cubic crystals were obtained, on gentle evaporation; the solution was therefore evaporated to dryness, and subjected to the action of repeated small portions of alcohol of the s. g. 811 ; and thus a quantity of muriate of soda was procured, weighing 1 grain.

E. The alcoholic solution was evaporated to dryness, the acetate thus obtained, was converted into a carbonate, by a red heat, in a covered platinum crucible; the carbonate was dissolved in water, evaporated to dryness, and weighed, while warm, 2 grains. Its solution gave a precipitate by nitrate of silver, indicating that a minute portion of muriate of soda had been dissolved by the alcohol; the carbonate of soda obtained may be safely estimated at 1.75 grains, and hence .25 of a grain must be added to the muriate of soda already obtained.

F. The insoluble matter B. was dissolved with effervescence in nitric acid, and the solution evaporated to dryness; when much concentrated, it became gelatinous, the dry mass was moistened with water, placed on a filter, and thoroughly washed with distilled water; the insoluble matter thus obtained, when dried, weighed .75 of a grain; it was of a greyish white colour, with a gritty feel, and fusible before the blow-pipe, with an alkali, into a glass, and was silex.

G, To the solution F. carbonate of soda was added, and after boiling a few minutes, the precipitate was collected on a filter, washed with distilled water, dried, and weighed, while warm, 18 grains; it consisted of carbonate of lime, and, on examination, gave no trace of magnesia.

H. The liquid from which the lime had been separated, was concentrated by evaporation, a precipitate appeared, which was redissolved in sulphuric acid, and precipitated by carbonate of ammonia and phosphate of soda, the precipitate dried and weighed, while warm, afforded 9 grains of the ammoniaco-magnesian phosphate, equivalent to 3.65 grains of carbonate of magnesia.

Hence, the solid matter contained in a pint of this water, consists of

Muriate of soda, (D. and E.) -	53.75	grains.
Carbonate of soda, (E.) - -	1.75	
Carbonate of lime, (G.) - -	18.	
Carbonate of magnesia, (H.) -	3.65	
Silex, (F.) - - - - -	.75	
	77.90	
Loss,	.1	
	78.00	

As the water had been taken from the spring some weeks, it was not attempted to ascertain its gaseous contents.

It is a question of some interest to know whether the substance procured from the water, actually existed in it, in the form which analysis presents them to us; are they not, in this instance rather to be considered as products of the operation, than educts? is it not probable that, in this water, the magnesia existed as a muriate, and that the quantity of carbonate of soda, is greater than that procured? Carbonate of soda and muriate of magnesia might exist in solution together, in this dilute state, without their mutual affinities being exerted, but when the solution was concentrated, carbonate of magnesia, muriate of soda would be formed; this idea is strengthened by the peculiar taste of the water, and its effects on the system.

It is more probable that the magnesia existed as a muriate, than that any portion of the lime was in that state; for although neither lime or magnesia were detected, after boiling, yet the quantity of magnesia is so small as to escape the operation of reagents, but a smaller quantity of lime than must be supposed to exist as a muriate in this case, if any, would be readily detected. I believe there is no mode of directly determining this point, as there is no reagent which will separate muriate of magnesia, or carbonate of soda. If, however, the idea that the magnesia exists as a muriate, is embraced, the proportions above stated must be altered, and the solid contents of the water be considered as consisting of

Muriate of soda, - -	48.57
Muriate of magnesia, - -	5.04
Carbonate of lime, - -	18.
Carbonate of soda, - -	6.36
Silex, - - - - - -	.75
	77.90

Boston, December, 1816.

On the opinion of Dr. Watt, in respect to the increased fatality of measles, since the introduction of vaccination.

[Communicated for the New-England Journal of Medicine, &c.]

DR. WATT, the Professor of the theory and practice of physick in Glasgow, has shown that the measles have become much more fatal in that city since the introduction of vaccination, than before. This remark has lately been noticed in our publick papers, and therefore calls for a few observations.

Dr. Watt does not suppose that the subjects of measles suffer more, because they have undergone vaccination. This is his explanation. Most children are affected both by small-pox and measles, unless in cases where the first of those two diseases, which attacks a child, proves fatal. Now the small-pox was commonly the first of the two; and this, he thinks, was fatal in all those subjects, whose constitutions were not well-fitted to endure these diseases. Those who resisted the small-pox therefore were, in almost all instances, able to resist the measles. But since the cow-pox has been substituted for small-pox, the measles have affected such subjects, as formerly were destroyed by small-pox, and has, in consequence, occasioned more deaths than formerly.

Before we admit this explanation of the increased mortality occasioned by the measles in Glasgow, we should inquire whether the same increased mortality from measles has been noticed in other places since the introduction of vaccination. Let us state the facts, in respect to this subject, which have occurred in our own vicinity.

A general inoculation for small-pox has not been permitted in Boston for twenty-four years. The same is true, in respect to almost every other town in New-England. Accordingly, very few persons among us, under twenty-four years of age, have undergone small-pox. The only exceptions, are among persons, who have been abroad, and a few who have gone into small-pox hospitals. During his period the measles have prevailed several times, and nearly all the subjects of it have been young children. Of these, some have been vaccinated, but very few have undergone small-pox. Accordingly, upon Dr. Watt's hypothesis, we ought to have found the measles a severe, and often a fatal disease. So far is this from being true, that it is not easy to find an instance of death from measles, where the patient has had a fair chance for medical assistance; and very few instances occur of death, from this disease, even where no regular attention is paid to it. Our own experience, then, is sufficient to show that Dr. Watt's opinions were not well-founded. The increased mortality from the measles in Glasgow, must probably be owing to causes, which have not been discovered, unless there be some difference in the constitutions of children there from those of our own vicinity. J. ———.

Boston, Sept. 1816.

Cases of Croup. *By William Gamage, jun. M. D.*

[Communicated for the New-England Journal of Medicine, &c.]

MARCH 29th, 1816, in the afternoon, I was called to Mr. H's. child, a girl about four years old. It was in extreme distress, and using every effort to breathe, the chest hove violently, and every muscle seemed called into action, to bring air into the lungs, and the small quantity thus obtained, passed into the trachea with a hissing noise, as if forced through a narrow opening. Frequent paroxysms of a shrill, ringing cough occurred, extremely violent, during which, suffocation seemed inevitable. Agony was depicted in its countenance; there was a deathly paleness contrasted with a flushed cheek, and the eyes were heavy and staring. The pulse was frequent and rebounding, but not much smaller than in health. It had been ill between five and six days, as the parents supposed, with a bad cold, with which it had been very hoarse. The present alarming symptoms commenced the day previous, at noon. A state of imminent danger, therefore, had continued at least twenty-four hours. There seemed but little hope of her recovery; but to afford the best chance, I proceeded to bleed her at the arm till she began to faint. The quantity of blood taken was over seven ounces. Respiration was immediately rendered easier, and continued so for some hours. Full vomiting was, however, produced as soon as possible, by the tartrite of antimony, and kept up for some time, by repeating the dose. At the same time, a large blister was applied to the chest and throat, and it rose well. The sub-Muriat. Hydrarg. two grains every hour was administered, till the bowels were thoroughly evacuated, and continued afterwards, according to circumstances.—30th, in the morning, the symptoms seemed to be in some degree mitigated, but at noon, as bad as on the preceding day. The pulse was not materially more feeble, and it was my wish to repeat the bleeding; but alarmed at the fainting in the previous operation, the parents would not consent. The emetic was repeated, and the calomel continued every two hours. A second blister was also applied to the back of the neck. Having heard the cicuta favourably mentioned, as a remedy in this complaint, by a gentleman, for whose opinion I have a high respect, I commenced giving it in the evening, at first half a grain every two hours, and augmented to a grain at a dose. Nauseating doses of squills and antimony were also administered. Bronchotomy had been proposed, but not assented to by the friends.—31st, symptoms no better, and the child fast sinking. Treatment not materially changed. She expired on the morning of the 1st of April. The body was subjected to examination six hours after death.

DISSECTION.

The larynx and trachea were carefully removed from their relative connexions, and were laid open by cutting on their posterior part. The mucous membrane, lining the larynx and epiglottis, exhibited marks of having been violently inflamed. It was of a highly florid complexion, and very much tumefied and corrugated. This state of the membrane was evident on the chordæ vocales, and arytenoid cartilages; and the rima glottidis was thus very much narrowed. No factitious membrane and a very little slimy matter was found in the larynx. But at the commencement of the trachea this membrane began. It was completely formed, and lined the whole trachea. It was, at least, a line in thickness, and so firm as to be roughly handled without tearing. It continued into the bronchiæ and their branches, and could be distinctly traced, of a firm consistence, as far as these were cartilagineous; but where this structure ceased, there suddenly terminated this membrane, as if cut off; the end was of considerable thickness. This is not the view of the state of a single tube, from six to eight of these branches were minutely traced in different parts of the lungs, and the same thing exhibited. In the membranous continuation of these tubes not the smallest vestige of the adventitious membrane was to be found; but an unnatural redness was perceived, evincing that the inflammation had extended here also. This redness was evident through the trachea and bronchiæ under the lining of the coagulable lymph. The body of the lungs had their natural complexion and character. There was no appearance of that thickening or consolidation of their substance, the common effect of pneumonia.

REMARKS.

Was the presence of the adventitious membrane, in any degree, the cause of the symptoms exhibited in this case? It appears to me, that it was not, even admitting it to have been formed at the commencement of the disease. For though it had acquired a considerable thickness, so as to diminish the caliber of the trachea, still a column of air could be transmitted sufficient, I conceive, at least to support life. The bronchia were proportionably more contracted, and this state would have formed an impediment to recovery, could other difficulties have been removed. But, to my mind it is clear, that the narrowness of the rima glottidis, in consequence of the swelling and corrugation of the mucous membrane, thus impeding free respiration, was the cause of the most distressing symptoms.

What might have been the effect of bronchotomy, at the time it was proposed, forty-eight hours after the symtoms began to be formidable, must be left to conjecture. But if, at that time, the lining of coagulable lymph had been formed to the extent, in which it was discovered after death, the operation, it is very certain, would have been of no use. It is not, however, an unreasonable supposition, that the membrane had not acquired that extent. Life, it will be remembered, continued forty hours after, a time quite sufficient for its deposition; and it seems, that in a great majority of those cases, which have terminated suddenly in death, no membrane has been found; whereas, it is pretty uniformly discovered in those, which have lingered for some time. In four of the five cases communicated by Dr. Jackson, and inserted in the first volume of this Journal, no membrane was found after death. It seems, then, to be only an accidental circumstance, and not a necessary constituent of the disease. From a view of the several cases on record, and from my own observation, I am disposed to think, that an inflammation of the mucous membrane of the larynx, trachea, and bronchiæ, but particularly of the larynx and its glottis, constitutes the proximate cause of this disease. I am aware that it has gotten the name of cynanche trachealis, and that the inflammation of the larynx, which has of late engaged the attention of the profession, in England, is, by learned gentlemen, considered a distinct disease, and has been named cynanche laryngea; and the celebrated Blane, late president of the Medical and Chirurgical Society, of London, seems to consider this as differing as much from croup, as from any other inflammation of the throat. But from the following considerations, it would seem difficult to establish any real difference betwen C. Laryngea and Croup. 1st. In every genuine case of croup, is there not inflammation of the larynx? Is there on record a single case of morbid anatomy, the effect of croup, in which the larynx is distinctly said not to have been inflamed? 2d. Will the inflammation of the trachea, independent of an accompanying inflammation of the larynx, produce the diagnostic symptoms of croup? If the phenomena are to be attributed to the presence of the adventitious membrane, as has generally been supposed, the trachea ought to be found nearly impervious after death. But was the coagulable lymph ever found accumulated to such a degree as to produce this effect?—Unless the above queries can be answered affirmatively, cynanche laryngea, I conceive, is the most appropriate name for croup, and the cases, which have excited so much attention in England, should be considered as examples only of a more acute form of the complaint, than generally occurs.

If the above view as to the nature of the affection be correct, much may be expected from early and liberal bleeding; and if bronchotomy is to accomplish any good, it should be delayed no longer, than it is necessary to obtain the full effect of bleeding. To anticipate the formation of the false membrane is a sufficient reason for doing the operation early. But it has failed in some cases, in which no membrane has been found after death. For such failures, two causes may be assigned. 1st. The depression of the vital powers, in consequence of the circulation of blood half changed from its venous state. 2d. The destruction of the function, whatever it may be, of the membrane of the bronchial tubes and the air cells, by the extension of the inflammation into these parts. In these instances of failure of the operation, the state of the bronchial tubes, particularly the minute branches, has not been noticed, a defect to be lamented in most of the cases of this disease examined after death.

In two days after the occurrence of the above case, another of the same disease, came under my care, the symptoms bore so strong a resemblance to the first, that it was impossible to mistake them. They differed in no respect, except severity. Going directly from one child to the other, I had the best opportunity of comparing them. This child was about two years old. It had been indisposed for three or four days with hoarseness and other appearances of a bad cold. Six or eight hours had elapsed since the commencement of the laborious and stridulous breathing. I proceeded to bleed it *ad deliquium*. Nearly six ounces of blood were extracted. The breathing was immediately relieved. Full and thorough vomiting, however, was produced by the solution of the tartrite of antimony, and the breast and throat were well blistered. Fourteen hours after my first visit, the symptoms were mitigated in a considerable degree; respiration was performed with more ease. The shrill cough, however, continued, and the child could neither cry nor speak, except in a whisper. The emetick was repeated, and the sub-Mur. Hydr. gr. ij. given every two hours till it acted on the bowels. The child gradually recovered; but it was some time before the cough ceased to alarm, or a loud articulation could be made.

Bleeding is now recommended, by most authors, as a remedy for croup; but the majority of them direct it in quantities so small, or with so many cautions against taking too much blood, that it is to be feared, if used at all, it is with such a sparing hand, or so late in the progress of the complaint, as to do no good. A disease so formidable in its character, must be arrested at once, or it is sure of its victim. The practice recommended by Dr. Ferriar seems the best calculated to command success in the treatment of this affection. The following extract from his medical histories, will exhibit his views: "In all cases of croup, which I have seen, I have found it necessary to bleed immediately; and when I have seen the patients sufficiently early, to entertain hopes of saving them, I have directed the evacuation to be continued so as nearly to produce faintness. This is the essential point of the cure, without which, no relief can be effected. Even if the patient should not be seen till the day succeeding the attack, it is proper to bleed *ad deliquium*, if the subject be plethoric, and the difficulty of breathing and restlessness be great." He advises the bleeding to be followed "by a blister, and a vomit of tartar emetic, in the usual doses, till full vomiting takes place." "If the first bleeding and emetic do not effectually relieve the cough and difficulty of breathing, it becomes necessary to repeat the bleeding, and the evacuation, in this case, must again be carried as far as the patient's strength will permit. A repetion of the emetic, after the second bleeding, often puts an end to the disease," &c.

Calomel has gained a reputation as a remedy for croup, and as an assistant to bleeding and vomiting, it is probably, a valuable acquisition. But having used it not only in the above, but in several other cases, I am convinced, it should hold only a secondary place, and should not, for a moment, be allowed to divert us from the aid of those great means of cure. No other remedy, it is probable, would have been suffered to come in competition with bleeding, had not the affection been supposed, by some writers, to be sometimes spasmodic in its character. If such distinctions exist, it has not been my fortune to witness them; every case, which I have dared to designate by this name, croup, has exhibited evidence of violent inflammation. I have, however, seen some cases, which bore so strong resemblance to croup, as, at first sight, to excite fears as to the character of the complaint; the ringing cough, particularly, being present. There are two children to whom I have been repeatedly called, on account of these attacks. One of them, has, on these occasions, this peculiar cough, accompanied with considerable difficulty of breathing. The attack is generally sudden and in the night; but a gentle emetic, pretty uniformly relieves them, at most, it is seldom necessary to do more than follow the emetic, by a cathartic. The stomach seems to be primarily in fault. May it not be in cases similar to these, that some boasted remedies have acquired their reputation.

Boston, Jan. 1817.

HISTORICAL EXTRACTS ON VARIOUS SUBJECTS.

[Communicated for the New-England Journal of Medicine, &c.]

An additional evidence, that bleeding has long been in use in difficult labours; taken from Dr. ROBERT BLAND's *able account of the invention and use of the* Lever of Roonhysen; (*made publick February* 16, 1790.) See London Medical Communications, p. 397—462.

'IT seems proper to consider the general progress of a labour in that class, which is usually termed laborious; 'that is, where the child, presenting with its head, is protruded so slowly, and with so much difficulty, that its own life, 'as well as that of its mother, are in imminent danger; and to 'prescribe a *general method of conducting women* through 'such *difficult* labours. A woman is to be treated as if suffering spurious pains, so long as the os uteri remains thick, rigid, 'and close; or, after being a little open, does not go on dilating. 'If she is hot, and the vessels are full, *she must be blooded*; 'the bowels must be emptied by glysters, and by small doses 'of salts, castor oil, or some other fit operant; and at night, a 'sufficient dose of laudanum should be given to procure rest. 'The diet, in the mean time, must be regulated, and the air of 'the room kept cool; and the woman must be admonished not 'to strain during the pains, but leave them to exert their natural force; and this process must be continued until the os 'externum, as well as the os internum are so relaxed, as to admit, easily, the passing two or three fingers (or the hand, if 'necessary,) into the vagina, to examine the situation of the 'head of the child, and to explore the capacity of the pelvis.' p. 454, 455.

Historical Extracts respecting the application of the Colchicum Autumnale, *(or Meadow Saffron,) and the* White Hellebore, *to the cure of the Gout.*

RESPECTING the first article, the following note, subjoined by Sir GILBERT BLANE, to his Paper on the subject of *the Health of the British Navy*, is curious.

'Among other instances, (says Sir Gilbert Blane,) that 'might be quoted, of the neglect and oblivion, and the 'future revival of useful medicines; one of the most striking, 'in the History of Physick, is that of the *remedy for the Gout;* 'which, within these few years, has acquired considerable celebrity; and, though it has been suspected of not answering 'to its original character, is now regaining the publick confidence.

'Demetrius Papagomenos, (a medical writer of Constantinople, in the *thirteenth* century, in a work de Podagra, ascribes to the *Hermodactyl* the same virtues as belong to the 'secret medicine above alluded to, (known by the name of '*Eau Medicinale d'Husson.*) It is sufficiently ascertained 'that the Hermodactyl is the *root* of the same plant as the '*Colchicum Autumnale* of modern botanists; and it is also ascertained, that it is to this last, that the medicine in question 'owes its virtue.

'The author is indebted to Sir JOSEPH BANKS, P. R. S. 'for the knowledge of these particulars. And Sir Joseph 'allows him to say, that he is satisfied with the accuracy of 'the preceding statement: And also that he has experienced, 'in his own person, all the beneficial effects that have ever 'been ascribed to this medicine.'

See London Medico-Chirurgical Transactions, 6. 560.

It is understood, that Major Rennell, so well known by his various publications respecting the East-Indies and geographical subjects, possesses not only the same opinions, derived from experience, respecting the eau medicinale; but that he agrees with Sir Joseph Banks as to the identity of the colchicum autumnale and the operating ingredient in the eau medicinale.

Before the recent discovery, above noticed, that the root of the colchicum autumnale (which root once bore the name of Hermodactyl) was the principal agent in the eau medicinale of M. Husson; Mr. Moore, surgeon, and brother of the celebrated General Sir John Moore, who was killed at the battle of Corunna; had led the publick to a strong suspicion, that white hellebore and opium contributed to the chief efficacy of the eau medicinale.

Accordingly we find, from Dr. Thomas Young's Treatise on Consumptive Diseases, that Poterius, formerly Physician to the King of France, knew the virtue of *both* medicines for the cure of the gout. 'Poterius (says Dr. Young) seems to 'have been well acquainted with the *hermodactyls.* He 'recommends the extract of this plant, *together* with that of '*hellebore,* as an immediate cure for the gout, to be followed 'by the diaphoretic powder.' p. 164.

Statement of Rates of Mortality, *taken under different circumstances; with* remarks *on the first article.*

No. I.

TABLE of the *annual* mortality of the different *counties* of Great-Britain; according to the returns of 1811.

Middlesex	1 in 36	Rutland	1 in 53
Kent	41	Suffolk	53
Warwick	42	Brecon	54
Cambridge	44	Cumberland	54
Essex	44	Westmoreland	54
Surrey	45	Wilts	54

York, E. R.	47	Hertford	55
Huntingdon	48	Oxford	55
Lancaster	48	Sussex	55
Buckingham	49	Bedford	56
Southampton	49	Derby	56
Mean of England	49	Radnor	56
Chester	50	Dorset	57
Durham	50	Leicester	56
Norfolk	50	Salop	17
Lincoln	51	Devon	58
York, N. R.	51	Hereford	58
York, W. R.	51	*Mean of Wales*	60
Denbigh	52	Gloucester	61
Nottingham	52	Carmarthen	62
Northampton	52	Cornwall	62
Somerset	52	Merioneth	62
Stafford	52	Montgomery	63
Worcester	52	Monmouth	64
Berks	53	Pembroke	64
Flint	53	Carnarvon	67
Glamorgan	53	Anglesey	72
Northumberland	53	Cardigan	73

The above table (which was originally extracted from publick documents exhibited by the British government) is to be met with, at p. 105, of a *Treatise on Consumptive Diseases*, by *Dr. Thomas Young*, F. R. S. and L. S.; published in London, in 1815.—Dr. Young accompanies this table, with the following remark. 'It is obvious, that those counties, which 'contain large *manufacturing* towns, exhibit a mortality wholly independent of their climate; while the natural salubrity 'of others (for instance, Cornwall) is, probably, rendered 'more conspicuous by their exemption from sedentary employ'ment.'

The table, however, leads to some observation, which it has not occurred to Dr. Young, particularly to notice.—Not only the annual deaths, are more numerous in *England*, than in *Wales*, (being one to forty-nine, in the former, and one to sixty, in the latter; but, generally, the *western* side of the island, beginning from Wales, and thence proceeding southward, to Cornwall, seems comparatively more healthy than the other parts, notwithstanding, Cornwall contains many miners. Mountains or hills, it will be observed, generally abound in these western parts; as, also, mild winds, and moisture proceeding from the Atlantic Ocean. The counties containing marshy districts, are likewise, comparatively, unhealthy; and that county, (Middlesex,) in which London is placed, seems to exhibit considerable mortality from the customary causes, attending a superabundant collection of people; independent of the influence of its manufactures, which are much inferior, however, in amount, to those in some other English counties.

It is not to be forgotten, also, as to the above table, that though Dr. Young states, that it respects *Great Britain*, yet that in truth, it only regards *England* and *Wales*.

No. II.

Sir Gilbert Blane, in a highly interesting paper on the health of the British navy, states, that the total mortality, [among British seamen,] from disease, may be considered as about 1 in 42; " which is about double of that of subjects of the same " age in *civil life*. It is greater (he adds) than even that of " of French *prisoners of war;* which, in 1813, was 1 in 55; " as appears by an account extracted from the public returns. " It is also higher than that of the garrison of Gibraltar, which " is only 1 in 49, exclusive of the years in which the epi- " demic fever prevailed, [in that garrison."]

N. B. The statement of the *French prisoners*, here noticed, is taken from the public accounts for 1813; and the author adds, that the following circumstances, which are entirely unconnected with their treatment, tended greatly to augment the mortality. " 1st. The want of exercise, and that " depression of spirits, which is inseparable from a state of " captivity. 2dly. The extreme profligacy of many of the " prisoners; some of whom, were so addicted to gaming, that " they staked and lost their clothes, and even the articles of " their subsistence. 3dly. The returns included the deaths " of prisoners, who had been reduced to the last extremity " after a sea voyage; during which, they laboured under sick- " ness and wounds; particularly, those from Spain, (towards " the end of 1813,) after the siege of St. Sebastian's and the " battles of the Pyrenees."

As to the *prisoners of war at Dartmoor*, in England, in 1811, (who are generally understood to have been *American*;) out of 6572, only 36 were in the hospital, and only one had died in the week preceding the return.

Sir Gilbert Blane says, ' that, if the mortality during the ' twenty years of the French revolutionary war, had been 'equal to what it was in 1779, the whole stock of [British] 'seamen would have been exhausted; in which case, men 'would not have been procurable by any bounties, however 'exorbitant; for it has been stated, that if the mortality of '1813, had been equal to that of 1779, there *would* have died, 'annually, 6674 men *more* than have *actually* died; which, in '20 years, would have amounted to 133,480; a number near'ly equal to the *whole* number of seamen and marines employ'ed in the last years of the late war."—See the *London Medico-Chirurgical Transactions.* Vol. vi. p. 520, 561—562, and 518.

No. III.

It appears by a return of the number of deaths which occurred among the *convicts* in Portsmouth, and in Langston Harbour, from January 1, 1805, to January 1, 1815; that the *total* number of deaths, for this average of ten years, taken on an average of 420 convicts at Portsmouth, was 77, and on an average of 320 convicts in Langston Harbour, was 40.

No. IV.

" The *Northampton Tables,* (says Sir Gilbert Blane,) give 'a mortality of 1 in 57, in this class of subjects, [viz. men of 'an age from 20 to 40 years;] and these [tables] were con'structed at a time when the general rate of mortality in En'gland, was greater than at present. From a computation, 'founded on the experience of the last ten years, in the *Equi'table Assurance Office,* it appears [by information furnished 'to Sir Gilbert Blane, by Mr. Morgan, F. R. S. Actuary to 'that office,] that the mortality of persons at this age, does 'not exceed 1 in 130. This calculation, however, is made on '*select* cases; none but good lives being insured. From the 'best computation that can be made, the mortality in this class 'in the general population of *England,* is about one *half* the 'mortality of *all* ages; and this being 1 in 49, [for England,] 'the mortality of subjects from 20 to 40, ought to be 1 in 98, 'but as the decrease of mortality seems to be chiefly in infants, '(certainly so in London,) and as consumptions have been ob'served to be more frequent of late, among young adults '(probably from a greater number of sickly children being 'saved;) this rate of mortality is perhaps too low:—it cannot 'be far wide (he says) of the truth, if we take it at 1 in 80."

—See *London Medico-Chirurgical Transactions.* Vol. vi. p. 519.

N. B. It should be stated, that the persons making application to have their lives insured at English assurance offices are, generally, such as do not labour; and whose appearance is healthy, and who have repute for good health among their acquaintance.

Retrospect of the progress of Medical Science, from January to July, 1816.

[From the London Medical Repository.]

ANATOMY, PHYSIOLOGY, AND PATHOLOGY.

SO little has been done in the department of Human Anatomy since our last Retrospect, that we have one English work only on this subject to notice, and one which does not even aim at much pretension to novelty. Its author, Mr. Armiger, presents it as the first part of a more extended work, which, under the title of "Rudiments,"[*] he intends to serve as an elementary guide for students commencing their inquiries in Anatomy and Physiology. The arrangement is sufficiently accurate, and the definitions perspicuous; but it would have proved much more useful to the student, had it contained references to the best authors: for although it points out a method of classing the information he may obtain; and leads him to adopt a mode of study likely to facilitate very much his researches; yet he is, still, left unassisted as to the best sources from which he can obtain more detailed information.

In France, Dr. Cloquet has collected, in his *Traité d'Anatomie Descriptive,*[†] all that is known on pure Anatomy. The work is professedly simple descriptive anatomy; touching neither on Physiology, Pathology, nor historical discussion; and has been written strictly according to his own ideas of Anatomical writing. "C'est," says he, "le scalpel à la main qu'on doit faire un ouvrage d'anatomie; c'est à ceux qui ont le scalpel à la main que je laisse surtout mon livre à juger."

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[*] *Rudiments of the Anatomy and Physiology of the Human Body,* &c. By T. J. Armiger, &c. &c. London, 8vo. pp. 52, 1816.

[†] *Traité d'Anatomie Descriptive, rédigé d'après l'ordre adopté a la Faculté de Médecine de Paris.* Par J. Hippol. Cloquet, Docteur en Médecine, &c. &c. II. Parties. 8vo. pp. 1118. Paris, 1816.

The following arrangement, which M. Cloquet has adopted, is that of the French anatomists of the present day. The whole of anatomy is divided in three classes; the *first* of which comprehends—1. the organs of loco-motion; 2. the organs of the voice; and, 3. those of sensibility: the *second*, 1. the organs of digestion; 2. the organs of respiration; 3. the organs of circulation; 4. the organs of absorption; and, 5. the organs of secretion: whilst the *third* is altogether confined to the organs of generation in the two sexes. The advantages of such an arrangement are sufficiently obvious, particularly to the student; whose time should be chiefly spent in the dissecting room. But, although we are willing to admit that a correct, and, if we may be allowed the expression, a mathematical knowledge of the organs, and their relative connexion, in point of situation, with each other, is absolutely requisite for the surgeon, yet, we have never been convinced that any disadvantage can result from the introduction of physiological and pathological illustrations into Treatises on Anatomy.

The work of M. Cloquet, however, is not a mere dry description of the forms of the organs, their connexions, and positions; but in preliminary remarks on each division of the arrangement, the structure of the parts to be treated of, and even their general chemical composition, is examined. Thus, after describing the physical qualities of *bone*, we are informed that it is composed of two elementary parts—an organized parenchyma formed of gelatin, which, with fatty matter, constitutes more than one half of the weight of bones; and an inert, salinoterrene matter, that fills the cells and meshes of the parenchyma.* *Cartilage* and *ligament* are formed of cellular tissue, filled with a gelatinous pulpy matter. The *synovial fluid* is of greater specific gravity than water, with which it readily mixes: it froths when agitated, and is thready and consistent like albumen; the existence of which, in it, is demonstrated by the action of caloric, alcohol, and the mineral acids., It also contains fibrine, soda in a free state, and muriate and carbonate of soda. *Muscle*, M. Cloquet remarks, may be regarded as a kind of reservoir of fibrine, to which it owes its contractility, and which is enclosed in the parenchyma of this species of organ, in the same manner as calcareous phosphate is enclosed in that of bone. The chemical elements of muscle are fat, albumen, much fibrine, osmazom,* phosphate and muriate of soda, phosphate of lime, oxyd of iron, azote, hydrogen, and oxygen. *Cellular tissue* is an assemblage of very fine moist, whitish plates, and filaments, which interlace and form irregular cells that communicate with each other. With such remarks M. Cloquet prefaces his description of each particular set of organs. The simple anatomical details are full, and extremely perspicuous: and the execution of the whole work authorizes us in regarding it as a valuable addition to the elementary treatises on the important subject of which it treats.

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* Contrary to his resolution of adhering strictly to pure Anatomy, M. Cloquet hazards the following pathological hypothesis: "Si on verse de la potasse dans l'acide où un os a été en partie dissous, la matière terreuse se précipite; enfin quelques maladies, le cancer entre autres, rendent les os très-fragiles, à cause de la proportion relativement plus grande de cette même matiere."—p. 11.

Notwithstanding the importance of an accurate knowledge of the anatomy of the body, in its healthy state, to every description of practitioners, yet, it must be allowed that, to the physician in particular, an acquaintance with the changes which disease produces is still more essential, adding certainty to their reasonings, and confidence in prescribing: but this undoubtedly implies a previous familiarity with the healthy structure of the frame. From the period, however, of Bartholin† to that of Baillie, although morbid anatomy has engaged the attention of some of the most illustrious men that the annals of medicine can boast of, a Morgagni, Lieutaud, Bonet, and Vicq-d'Azyr, yet it may still be regarded, in some degree, as a province rich in many unexplored treasures; and, consequently, as every new work on the subject must excite a lively interest, we feel pleasure in being able to notice Dr. Cruveilhier's "*Essai sur l'Anatomie Pathologique.*"‡

Dr. Cruveilhier has divided his work into three parts. The first part, which treats "of Pathological Anatomy in general," contains, besides some preliminary remarks and a sketch of the history of this branch of the science, four sections: the ***first***, on mechanical injuries; the ***second***, on organick transformations, productions, and degenerations; the ***third***, on irritations, atonies, and gangrene; and the ***fourth***, on fevers and the neuroses. The second part treats of "transformations and particular organic productions:" and the third part, "of organic changes connected with hernia." This latter part is also subdivided into two sections; the *first* of which relates to "hernia, the constituent parts of which have suffered no change in their organization;" and the *second* to "the changes produced on both the containing and the contained parts." The diseased affections treated of under these heads, are arranged in classes, orders, and genera. Thus the section on Mechanical Injuries comprehends ten classes: 1, wounds; 2, ulcers; 3, fistulas; 4, contusions which comprehend concussions; 5, lacerations and distensions; 6, fractures; 7, displacements; 8, the presence of foreign bodies; 9, aneurisms; and 10, vices of conformation. It is unnecessary to go through the whole of the arrangement: from this specimen the extent of the subject embraced in the pages of Dr. Cruveilhier, may be readily imagined.

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* This is a particular animal principle, coloured and soluble in alcohol, which Thenard discovered, and ascertained to be the substance which gives taste and odour to broth (bouillon.)

† Thomas Bartholin published the first treatise which appeared on Morbid Anatomy in 1674, under the title "*Consilium de Anatomia practica ex cadaveribus morbosis adornanda.*" Hafniæ, 4to.

‡ *Essai sur l'Anatomie Pathologique en général, et sur les transformations et productions organiques en porticular:* par Jean Cruveilhier, Docteur en Medicine, ancien Elève interne de l'Hotel-Dieu de Paris, &c. &c. 2 tomes, 8vo, Paris, 1816.

In point of execution, Dr. Cruveilhier's work is undoubtedly too succinct in details to be of much advantage to the student; and may rather be regarded as a well-arranged collection of memorandums for the use of the already-formed practitioner. Thus we may illustrate, by the following short paragraph, all that he advances on the subject of synovial cysts, which constitute the second genus of a sub-order, intitled "of cysts existing prior to the matter they contain."

"GENUS II. *Synovial Cysts.* These are cysts that contain a ropy liquid, as limpid as synovia. They are observed only round the articulations of the hands, the feet, sometimes the knee, and along the sheaths of tendons. Some authors think that they are the consequence of the synovial fluid escaping at one point: but observations on the dead body demonstrate that these cysts have dense parietes, fibrous externally, and internally serous, formed in the cellular tissue which surrounds the true synovial cysts. The laceration of the cysts is the method of cure most generally adopted: but incision, excision, and even extirpation are also practised."

Dr. Nathan Young, of Edinburgh,* has published an account "of a singular malformation of the human heart.†" The organ was about twice the natural size, for a man of ordinary stature: it weighed, when freed from the coagula and with its vessels cut short, twenty-eight ounces and forty-four grains. The auricles formed one extensive cavity, by the dilatation of the foramen ovale, which was open and measured three inches and a half in diameter. The cava and pulmonary veins were much enlarged, as were also the eustachian valve and the great coronary vein. The size and thickness of the pulmonary ventricle agreed with the size of the heart. "The ostium was two inches and half in diameter; the tricuspid valves were partially ossified and much thickened. The pulmonary artery much exceeded its natural size, and was larger than the aorta:" its semilunar valves were completely ossified. The parietes of the aortic ventricle were thicker than usual: its ostium was an inch and a half in diameter; the semilunar valves were a little thickened, but all the other parts were natural.*

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* Tome i. p. 296. † *Journal of Science and the Arts*, vol. i. p. 49.

Considering the facts of the case, Dr. Young is induced to advance the following *queries*. 1. "What must have been the action of the heart in this case, so as to keep up that equable circulation which existed for such a length of time in defiance to the extensive malformations? 2. Does the morbus cœruleus depend on a mixture of venous and arterial blood? 3. Wh[illegible] portion of venous blood is requisite? 4. Is it not more probable that the morbus cœruleus depends on languid circulation? 5. Is the ductus arteriosus generally found pervious in cases of the morbus cœruleus, in combination with an open foramen ovale? 6. Is it necessary that the ductus arteriosus and foramen ovale be open in the same subject to constitute this disease?

On this query we may remark, that the foramen ovale is very frequently found open on the dissection of adults, in whom no symptoms have appeared during life that could have led to the suspicion of the existence of such an event. Dr. James Thomson, of Edinburgh, has endeavoured to explain this circumstance, by observing, that "as long as both auricles receive and discharge the same quantity of blood, no admixture to any extent can take place, even although the foramen ovale be very large; but when the right becomes either smaller or of increased strength, or the left is seized with what is called passive aneurism, a portion of the blood must go through to support the equilibrium."*

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* The patient in whom this case occurred, was a labourer, forty-nine years of age. During the last eighteen years he had had repeated attacks of inflammation of the thoracic viscera: and an apoplectic attack, which had left a general sensation of numbness. He had a livid complexion; but no other symptoms of a diseased heart were perceptible, except the state of the pulse which was frequent, intermittent, and variable in frequency and strength. The temperature of the body varied from 97° to 101° of Fahrenheit: but he nevertheless frequently complained of coldness of the extremities. As he had laboured under eczema *mercuriale*, these symptoms were supposed to arise from *erethismum*: and he was accordingly treated with antimonials, opium, sarsaparilla, cinchona, and mineral acids. *Journal of Science*, Vol. 1. p. 49.

6. Is it not probable that a slow circulation through the lungs, which must have taken place in this case, may give rise to superoxygenation of the blood, and hence the effect of admixture of venous and arterial blood be obviated. 7. Is there not an equilibrium in the action of the cavities of the heart, which in some cases of malformation compensates for natural structure, and which, as soon as it is subverted, gives rise to symptoms indicative of morbid structure? 8. What are the pathognomonic symptoms of this malformation, or complete dilatation of the foramen ovale, contracted ostium arteriæ pulmonalis, and extensive enlargement of the heart?"

No part of *Pathology* has been more elucidated by the great attention that has of late years been paid to morbid anatomy, than diseases of the heart. Among the dissections of that organ in a diseased state, which have been lately published, one of the most remarkable occurred in the practice of Dr. Thomas Spens. A large portion of the anterior surface of the heart was found covered with a loose flocculent matter, adhering at one or two points to the pericardium; and a large portion of the anterior of the parietes of the right ventricle was converted into firm whitish curdy matter, resembling that which is found in scrophulous tumours.† In a case detailed by Dr. Pearson,‡ the mitral valves were partially cartilaginous and ossified, and the semilunar valves indurated, so as to obstruct the passage from the left ventricle to the aorta, and occasion accumulation in the left heart."

Dr. Duncan, jun. has published the dissections of three cases of inflammation of the heart, or rather of its membranous coverings, one of which he regards as an "example of pericarditis, nearly as free from complication as it can occur."§ In this point of view, it would indeed be a record of great value but we are of opinion, however, that neither the symptoms nor the dissection of the body after death authorize the observation; and that the case was a complication of pleuropneumonia and carditis. The dissection is interesting: "few or no adhesions were observable; nor was there any considerable effusion of serum into the sacs of the pleura. A little to the right of the sternum there extended from the lower part of the left side a dirty chocolate-coloured bag, which, on being opened, proved to be the pericardium adhering to the lungs, thickened and much distended; also containing two pounds six ounces of perfectly formed pus." The inner surface of the pericardium, "and that which is reflected over the heart, were thickly coated with a substance resembling, 'condensed curds;' the substance of the heart was not otherwise altered, except that it was much paler than usual."

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* *Edinburgh Med. and Phys. Journ.* vol. xii. p. 6.

† *Edinb. Med. & Phys. Journ.* vol. xii. p. 192.

‡ Ibid. p. 195. § *Edin. Med. and Surg. Journ.* vol. xii. p. 70.

We cannot avoid condemning, *en passant*, such loose expressions as "few or no adhesions;" there must have been either some adhesions or none.

The instance of malposition of the stomach, which we published,* on the authority of Dr. Joseph Benjumeda, of Cadiz, is more curious than important; and the facts are scarcely sufficient to authorize the conclusion of the Spanish physicians, that it was a *natural* malposition.

We cannot close, more appropriately, this brief sketch of what has been done in anatomy, within the period which this Report embraces, than by noticing the history of the art, by Dr. Lauth, Professor of Anatomy at Strasburg.† The first volume only is yet before the public. The *first* book treats of the anatomy of the Egyptians, who, notwithstanding the assertions of some authors, and their skill in the art of embalming, had scarcely, in any degree, cultivated the art. The ancient Greeks and Romans were conversant with comparative anatomy, but had made but little advancement in that of the human body. The anatomy of Galen was of the same character: and it was not until the period of Vesalius, in 1545, that the art began to assume its more important feature as displaying the structure of the body of man himself. In the middle ages scarcely any advancement was made: the writings of that period are defaced by the grossest errors: and, with a useless display of learning, in collecting the opinions of their predecessors, they examined and treated the subject itself in the most superficial manner. Dr. Lauth, in the *sixth* book, which relates to the Italian school, states, that although materials sufficient to form a system of pathological anatomy were collected in Italy, yet none was yet produced; and the work of Schrenk must be regarded as the first on that branch of the art. The volume concludes with several interesting biographical sketches of the most celebrated of the anatomists who are previously mentioned; and the manner in which the whole is executed, affords the best earnest of the value of those parts which are yet to come.

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* *Repository*, vol. v. p. 423.

† *Histoire de l'Anatomie*, par Thomas Lauth, M. D. &c. &c. tome i. 4to. Strasburg, pp. 606.

In *Physiology*, which always presents a wider and more inviting field of inquiry to the ingenious mind, several works of very considerable interest have appeared.

As regarding the subject in a general point of view, the most important work we have to notice is M. Majendie's *Elementary Treatise*,* the first volume of which only is yet published. It contains what he terms the *relative functions*, the organs of sense, of understanding (*intelligence*,) of the voice, and of motion. The subsequent volume is intended to contain the nutritive and generative functions. The style in which this work is written is particularly pointed and perspicuous, and well calculated for its object, being intended for students. Its great merit seems to consist chiefly in the author not having thought it necessary to introduce all that he could copy from the other writers on the subject; but having confined himself to the detail of what he conceives to have been satisfactorily ascertained by others, or to be proved by his own researches. Respecting vision, the following are original remarks: 1. That the secretion of the Meibomean glands is albuminous, soluble in the tears, and not unctuous as has been hitherto described. 2. The sclerotic and choroid coats of the eyes of albino animals (such as rabbits and pigeons) being transparent, shew, in a very easy manner, images of objects on the retina, which, in other eyes, required a tedious and difficult dissection. He has also been enabled, by the aid of these eyes, in making some experiments by evacuating the aqueous humour, removing the lens, cutting away the cornea, or enlarging the pupil, to observe the effects of these operations upon the images of objects represented on the retina. He has, also, enlarged his researches on the organ of the voice, some of which (the uses of the epiglottis) have already been noticed by us.[*] In grave sounds, the ligaments of the glottis of a dog vibrated in the whole of their length, and the air was expelled through the entire extent of the glottis: in acute sounds these ligaments vibrated only at their posterior part, and the air was expelled only through the portion of the glottis which vibrated.

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* *Precis Elementaire de Physiologie*, par M. Majendie, &c. &c. 1 tome, pp. 326, Paris.

For further particulars we must refer our readers to the analysis of the work in our present number. It is certainly an acquisition to Physiology, as there is no original elementary treatise in English on the subject, adapted for students; and Richerand's, which is filled with much dull doctrine that nobody believes nor ought to believe, is almost the only treatise accessible to the English reader.

Although two centuries have nearly elapsed since the splendid discoveries of Harvey ascertained the course which the vital fluid pursues through the animal frame, yet, physiologists are still divided in opinion as to the causes by which it is moved in that course. One set maintain, that the projectile power of the heart, aided by the vibrations of the arteries, is perfectly adequate both to transmit it to the extreme vessels, and from these to return it back again to the heart; whilst another set, with whom more recent physiologists agree, contend, that the impetus of the heart cannot carry it beyond the arterial system; and that its motion in the veins " arises from causes distinct from those by which it had been produced in the arteries." Dr. Carson has entered upon the investigation of this subject with the true spirit of philosophical inquiry, and published the results in a work which we have now to notice.[†] It is divided into three parts: " 1. An enumeration and estimate of the causes which have been supposed to promote the circulation of the blood. 2. The development of other causes which appear to contribute to the motion of the blood, and which, added to the preceding, will, it is presumed, be found adequate to the effect; and, 3dly, an application of phenomena."

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[*] Vide *Repository*, vol. v. p. 155. M. Majendie having investigated the nature of the action of the œsophagus in vomiting, has, in the Memoir to which we have referred, examined that organ in a state of rest, or nearly approximating to rest. He observed that the œsophagus has a vermicular motion, which extends, however, to two-thirds only of its length, the lower third being scarcely affected by it. This action ceases altogether when the eighth pair of nerves is divided. It is more energetic when the stomach is distended with food: and when that organ is compressed: and, even if the superior two-thirds be taken away, the remaining third, if the plexus of the eighth pair of nerves be not injured, when irritated is sufficient for the excitement of vomiting.

[†] *An Inquiry into the Causes of the Motion of the Blood; with an Appendix, in which the Process of Respiration, and its Connexion with*

The first part does not demand any particular notice. After describing, perhaps, in too detailed a manner, the various organs which compose the circulating system, Dr. Carson examines the theories proposed by Harvey,[*] Weitbrecht,[†] Muschenbrock,[‡]

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the Circulation of the Blood, are attempted to be Elucidated. By James Carson, M. D. Physician to the Workhouse. 8vo. pp. 250. London.

[*] After Harvey's opinion, "that the auricle is dilated by the force of the blood returning to it from the veins," was admitted, the following query naturally suggested itself: "Whence does the blood, at the root of the vena cava receive such a quantity of motion, as is not only sufficient to carry it with great velocity along the channel of the vein, but, in addition, to empower it to resist the contraction of the auricle, and to distend the cavity of that chamber? Dr. Harvey, and many of his followers, contended that this power is derived from the left ventricle of the heart, extending through the aortic and venous systems back to the right auricle."—Vide *Inquiry*, p. 25.

[†] Weitbrecht was a physician in Petersburg. He first advanced the opinion, which has since his time been generally admitted, that the propelling power of the heart alone cannot produce the dilatation of the whole of the arterial system; and that the circulation is kept up in the arteries, by the jet of blood sent from the ventricle acting on their irritability, and also enlarging the portion of their cavity, into which it is thrown, so as in both cases to occasion a contraction and recoil upon the blood, and restore to it the impetus that had been expended in dilating the artery. But the *vis a tergo* of the extreme arteries is inadequate to continue the motion of the blood through the veins to the heart.

[‡] Muschenbrock's theory was founded on the idea, that the blood-vessels constituted "a cone, with the base at the heart, and the apex formed by the extreme vessels." He contended, "that the force transmitted from the heart and arteries, would be in proportion to the square of the diameter of the small vessels, which were supposed to constitute the apex of the cone (and therefore in them smallest) multiplied into the distance of the section from the part."—*Inquiry*, p. 55.

Dr. Darwin,[*] Dr. Wilson,[†] John Hunter,[‡] and others; and proves, in a clear and satisfactory manner, the inadequacy of the causes enumerated by these celebrated physiologists, to produce the whole motion of the blood: hence, in the second part, he proceeds to point out the causes which, in his opinion, are capable, in cooperation with the former, of completing the circle of the vital fluid. Dr. Carson, in advancing his theory, first investigates the structure of the heart itself, and concludes, that "there arises, from the circular direction of the fibres of the heart, and the arched forms of its walls, a very remarkable property, that of dilating or expanding its cavities by the simple relaxation of its fibres." (p. 97.) The auricles differ in structure as well as in appearance from the ventricles; and there is no communication of substance between them: "for if the heart be boiled for a proper time, the auricles will drop off, or be easily separated from the ventricles, without any rupture of muscular fibres." Much stress is placed, as a preliminary remark, on the ordinary condition of the lungs, which the author conceives as an unnatural state of expansion, commencing the first moment the infant breathes; and the power which produces this state, is the weight of the atmosphere. But there is a powerful inherent disposition in the lungs to collapse, and to draw after them the parts which form the internal walls of the chest, of which the external surface of the heart is regarded as constituting a part.

On these observations chiefly Dr. Carson imagines, that the dilatation of the heart, after contraction, is "to be ascribed to the form and position of its fibres, in consequence of which, simple relaxation is accompanied by a certain degree of dilatation; but particularly to the supporting of a part of the atmospherical pressure that would have rested upon the convex surfaces of the heart or its envelope, by the resilient or collapsing effort of the lungs." By the dilatation thus produced, the valves "at the roots of the arterial trunks," owing to external pressure become closed, and the blood cannot return; but the passage from the auricles into the ventricles being open, the blood, by which the former were dilated, rushes in to occupy "the space left by the dilating ventricles." The communication between the auricles and the venous trunk being now opened, the blood in these trunks being relieved "from a part of the ordinary pressure in the direction of the heart, necessarily takes the course where it meets with the least resistance." The heart, therefore, acts at once in a two-fold capacity. By the contraction of the ventricles, it propels the blood through the arteries; and by the dilatation of the auricles, it pumps it from the veins. It is, at the same time, a forcing and a suction pump." But this power alone would be insufficient for raising the blood through the veins to the heart; hence the aid *vis a tergo* of the arteries is required; and by the cooperation of these two powers, it is moved in a continued stream from the remotest parts. Thus the motion of the blood in the veins "is produced by the force of the heart and arteries urging it behind; by the abstraction of a share of the atmospheric pressure from it in front, in consequence of the resiliency of the lungs, interposing its [their] influence in the intervals between the contractions of the heart; and by the gravity, which is rendered available in this case by the projection of the arteries and the diastole of the heart." (p. 151.)

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[*] Dr. Darwin maintained, that the veins take up the blood from the arteries by absorption, as a sponge imbibed water, but did not inquire into the cause of this absorption.

[†] Dr. Wilson explains the motion of the blood in the veins on the supposition, that the heart dilates, owing to a property inherent in it, and produces a vacuum, "by which the blood is pumped from the veins into the heart."

[‡] J. Hunter's theory was founded on his doctrine of the vitality of the blood.

Such is the outline of the theory of Dr. Carson, on the merits of which, as we have not yet had sufficient time to reflect, we forbear from giving any decided opinion.* The *third part* contains many interesting observations, in explanation of phenomena, which our limits prevent us even from noticing. An appendix also is added, advancing some interesting observations on respiration, and the action of the lungs.

It has been a question in the physiology of parturition, whether there is such a circumstance as a *spontaneous evolution of the fœtus*, "or in the event of the arm being the presenting part, whether a child, either living or dead, can be brought into the world by the efforts of Nature." Experience has proved that the efforts of Nature are, in some such cases, adequate to the birth of the child; but a difference of opinion exists as to the mode by which it is accomplished: and, as this is a question which has been connected with the practice of midwifery, the solution of it is of considerable importance. This operation of nature, the spontaneous evolution, was first pointed out by Dr. Denman, and he founded upon it a practical rule, that when a child cannot be turned without some danger to the mother, the chance of an evolution should "set our minds at ease," and induce us to leave the case to Nature. Dr. Douglas combated these opinions; denied that any *evolution* occurs; that a *recession of the extremity* is impossible; and maintained that the child is simply expelled in a doubled state, by the efforts of the uterus, as in ordinary labours. In an *essay* upon this subject,* Dr. John Kelly has freely criticised the opinions of both these eminent men. He admits that the evolution may occasionally occur, but it produces no recession of the presenting part into the contracting uterus, and gives the following explanation of the mode in which it is performed:

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* One passage, however, we feel inclined to extract: "It follows, from what has been stated, that this earth is not habitable by animals above a certain magnitude; and, that, as the weight of the column of fluid, contained by the animal machine, approaches nearer to a balance with the weight of the atmosphere, the circulation through the machine must be more languid. Little animals appear to be best adapted to the atmosphere of high mountains; accordingly we

"The inferior extremities are forced gradually lower, until they become, as it were, fixed upon a plane, with the body of the child; its position in the uterus from being perpendicular becomes horizontal, the head resting, I will suppose, on the spine of the *pubis*, and the interior extremities at the *sacrum*. It is when brought into this situation by the repeated action of the uterus, and not sooner, that the *fœtus* can turn upon its own axis; nor can it do so then, until the action of the uterus has ceased: so long as this continues, it is physically impossible the evolution can occur; but the child being thus placed, by the pains, in a position favourable to its evolution, the superior extremity goes up the moment the *uterus* ceases to press it down, or, in other words, at the termination of a pain." (p. 11.) But although Dr. Kelly admits that spontaneous *evolution* may in this manner take place, yet, he contends, that it may frequently be prevented by occurrences connected with the form of the fœtus. Thus, as the axis of the child's body

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find that animals which live upon very elevated mountains are less than those of the same species which dwell upon the plains." Page 203.

* *An Essay upon the Spontaneous Evolution of the Fœtus*, by John Kelly, M. D. 8vo. pp. 60. Dublin, 1816.

("as tending to an evolution,") is a line passing from the *umbilicus* to the opposite *dorsal vertebra*, a due proportion of every part of the fœtus is essential to an evolution: and, therefore, if the head be unusually large and weighty, none will ensue. Evolution may, also, be prevented by variations in the natural "relative dimensions of the uterus and the child;" by that state of the uterus which has been denominated its *permanent contraction;* by deformity of the pelvis, and several other causes. The practical inferences, which Dr. Kelly very properly draws from these observations, may readily be conceived.

Connected with the œconomy of fœtal existence, in another point of view, we have to notice the observations of M. Dutrochet "*on the Envelopes of the Fœtus*."* This author, with unwearied care and a scrutinizing eye, watched the process of incubation through all its stages. On the second day, he observed on the surface of the yolk a vascular areola, in the centre of which the first rudiments of the chick was perceptible. The whole of this yolk, or vitellus, is surrounded by two non-vascular membranes, which M. Dutrochet denominates the first and second epidermis, and under which is the vascular membrane that Haller discovered to be an appendix of the bladder of the fœtus. It is not until the fourth day of incubation, that the vitellus, which enlarges by degrees, bursts its first envelope and casts it off. In the course of the same day, the membrana allantoidis appears, proceeding from the abdomen of the chick by an opening situated under the median line. This allantois is filled with a yellow fluid, which is urine: it quickly developes itself, bursts the second epidermis which encloses it; and, attaching itself to the exterior membrane of the egg, continues to extend itself, sliding between that membrane and the albumen; so that on the tenth day of incubation, the whole of the egg is found enveloped by the allantois thus expanded, which forms new coats to the egg that were not in existence at the commencement of incubation. The exterior, which is the chorion, performs the function of respiration; the second is extremely delicate and analogous to that in the fœtus of the mammalia, which Haller denominates middle membrane (membrane moyenne.) M. Dutrochet proves, contrary to the opinion of *Haller*, that the vitellus is not originally surrounded by its vascular tunic, but that this membrane, which is an appendage of the intestine, covers progressively the yolk, in the same manner as the albumen is enveloped by the allantois. M. Dutrochet has also discovered that the vitellus possesses a herniary sac formed entirely from the peritoneum. This sac, which is drawn into the abdomen towards the end of incubation, receives some extremely minute vessels, that had escaped the notice of former observers, and which arise from those of the vitellus. From these observations it appears that the chick both respires from the first moment of its existence, and is nourished exclusively by the intestinal membrane of the yolk; and afterwards respiration is carried on by the membrana allantoidis: hence there are two phases, as it were, in the respiration of the chick.

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* We gave a short notice of M. Detrochet's Memoir in our last Retrospect. We have not seen the original; and our information regarding it is taken from the Report of M. Chaumaton, made to the Societé Médicale d'Emulation, published in the *Journal de Médecine*, tome xxv. p. 89.

The eggs of reptiles, M. Dutrochet observes, closely resemble those of the chick, except that the egg of the serpent contains no albumen. The egg of the viper presents several remarkable phenomena: it continues in the oviducts until the young are hatched; and towards the middle of gestation, which continues about four months, the exterior membrane of the ovum disappears, and the chorion remains naked in the oviducts, to which it slightly adheres; so that probably by this means the foetus derives some nourishment from the mother.

M. Dutrochet asserts, that both Swammerdam and Spallanzani have fallen into great mistakes regarding the ova of the frog genus. He has repeated the experiments of the Italian Naturalist on a great number of species, and is convinced that the produce of generation is a true egg. He has also proved, that the metamorphosis of the tadpole does not happen as Swammerdam has stated, by the casting of the skin which covers the fore legs, but these, furnished with their proper skin, pierce the membrane which covers them, and pass through it like the arms in a cuirass. In a few days the rent skin again adheres at the shoulders. The skin which covers the body and the hind legs of frogs is not the same which covers their fore legs: hence M. Dutrochet regards adult frogs as animals retaining the amnios during life.

M. Dutrochet draws an analogy between the ova of the mammalia and of birds. He regards the deciduous membrane of Hunter as analogous to that lining the shell of the eggs of birds; and under it, as in the egg, is the chorion, which is also an appendix of the bladder. The interior epidermis, which extends with the mucous membrane of the bladder of the fœtus, and is in contact with the urine, has been described by all anatomists under the name of allantoid. M. Dutrochet has found in the fœtus of the sheep, as well as in that of birds, a middle membrane covering the amnios without adhering to it; and he has observed the umbilical vesicle adhering to the lateral part of the small intestine, in the same manner as the vitellus adheres to the intestine of the chick. This vesicle is furnished with two long tubular horns, which might be erroneously taken for chalazes. In the earlier stage of the existence of the embryon, there is no placenta, and the embryon appears to be nourished by absorbing the fluids secreted by the mother; but as soon as the chorion begins to redden in the places where it is pressed by the tuberosities with which the uterus of the sheep is furnished, these red spots become the rudiments of placentas.

Connected with the subject which forms the labours of M. Dutrochet, are the observations of Mr. Ireland, on the early changes in the Rana *paradoxa* of Linnæus, or the Surinam frog.* In the tadpole state this animal resembles a fish; but on being narrowly examined, two small legs are perceived immediately behind the head. In a fortnight these are evolved, and the body of the animal enlarges: in three weeks the fore legs appear, and the head becomes very distinct; the animal acquires great activity, and requires frequently to ascend to the surface of the water to respire. This necessity for supporting respiration in the air, continues until about the sixth week, at which time the tail sloughs off, and is partly absorbed, and the animal becomes a perfect frog. Sir Everard Home, who has anatomically examined these animals, found, that before their transformation, there are three gills on each side, and the lungs remain "in a very small form in the posterior part of the abdomen behind the liver:" in this state also nearly the whole cavity of the abdomen is filled with fat; but as the animal becomes older, a considerable diminution of the capacity and the length of the intestines take place. The subject certainly well merits further investigation.

Dr. Wilson Philip continues his efforts to illustrate the functions of the nervous system. In a paper read to the Philosophical Society in January last, an account of some experiments on the nervous influence in secretion are detailed. He has demonstrated that the function of secretion is altogether dependent on nervous influence; for when this is interrupted, secretion is at an end. No digestion took place in the stomachs of rabbits when the eighth pair of nerves was divided: but efforts at vomiting, and a state of dyspnœa succeeded, which at length produced suffocation. Dr. Philip has also endeavoured to ascertain, by experiment, that the galvanic influence, if not the same as the nervous, is at least capable of supplying its place: a very curious fact, if it be borne out by further experiments. He regards it likewise as proved, that the ganglia communicate to the nerves, which proceed from them, the general influence of the brain and spinal marrow: and that the heat of animals is owing to nervous energy.

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* *Journal of the Sciences and the Arts*, vol, i. p. 54.

CHEMISTRY.

The value of the atomic theory, in promoting a more perfect knowledge of the general principles of chemical science, is daily becoming more conspicuous. Dr. *Prout* having advanced an opinion, that, "the specific gravity of any body may be obtained, by multiplying the weight of its atom, by half the specific gravity of oxygen gas," Dr. *Thomson* has published some interesting observations, tending to exhibit, "at one view, the very simple relations which exist between the specific gravities of gaseous bodies, and the weight of their atoms."[*] He has divided all the substances which can be exhibited in a gaseous state, and the weight of the atoms of which are known, into three sets, and exhibited them in a tabular form,[†] by which it appears, that "the weight of the

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[*] Annals of Philosophy, vol. vii. p. 343.

[†] Supposing the specific gravity of a volume of oxygen to be 1·, gaseous bodies may be arranged in three sets, as follows :—

Set First.—Bodies having the weight of their atoms equal to the specific gravity of their volumes.

	Sp. gr. Oxygen being 1·	*Weight of an Atom.*
Oxygen	1·000	1·000
Olefiant gas	0·876	0·876

Set Second.—Bodies having their weight of their atoms twice the specific gravity of their volumes.

	Sp. gr. Oxygen being 1·	*Weight of an Atom.*
Phosgene gas	3·095	6·190
Chlorine	2·250	4·500
Sulphurous acid	2·000	4·000

atom is either equal to the specific gravity of the gas, or twice that weight, or four times that weight:" hence, as the ultimate atoms of bodies differ in their weight, "the ratio of their weights may be determined by the specific gravity."

It has been generally regarded as a law in the doctrine of affinities, that no chemical action takes place, unless one or both of the bodies brought together be in a state of solution; yet, Berthollet's theory admits "that a chemical combination is produced, by the trituration of dry bodies together." Mr. H. F. Link has submitted this to the test of experiment, and published his observations in Schweigger's Journal.* His experiments were made on muriate of lime, triturated with sulphate of copper; crystallized acetate of lead, with anhydrous sulphate of copper; acetate of lead, with burnt alum; and several other similar salts, in none of which decomposition was effected, until water was poured on the mixture. Litmus, dried by heat, underwent no change of colour, when triturated with succinic acid; even when alcohol was poured on the mixture, it did not become red until the spirit was evaporated, and then the change was effected by the alcohol facilitating the absorption of moisture from the air. But when sulphur was triturated with phosphorus, the two bodies united

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Cyanogen	1·621	3·242
Nitrous Oxide	1·375	2·750
Carbonic Acid	1·374	2·748
Sulphuretted hydrogen	1·062	2·124
Sulphur	1·000	2·000
Azote	0·875	1·750
Carbonic oxide	0·875	1·750
Steam	0·5625	1·125
Carburetted hydrogen	0·4995	0·999
Carbon	0·375	0·750
Hydrogen	0·0625	0·125

Set Third.—Bodies having the weight of their atoms four times the specific gravity of their volumes.

	Sp. gr. Oxygen being 1·	*Weight of an Atom.*
Hydriodic acid	3·986	15·944
Muriatic acid	1·1557	4·623
Nitrous gas	0·9375	3·750
Hydro cyanic acid	0·8433	3·3732
Ammonia	0·53125	2·125

* Vol. xiv. p. 193. There is a translation of this Paper in the *Annals of Philosophy*, vol. vii. p. 426, from which we quote.

into a yellowish liquid, evidently the consequence of the heat evolved during the trituration.

The following are the conclusions which Mr. Link draws from his experiments:

"1. The trituration of anhydrous bodies produces no chemical action. 2. But it takes place, when one or both bodies are soluble in the liquid poured upon them. 3. It takes place equally, when one of the bodies only is soluble in the liquid poured on the mixture. 4. The water of crystallization acts as free water; but the moisture of the atmosphere acts only when it is absorbed by one of the triturated bodies. 5. The consequence of the decomposition has no effect on its success. It is the same thing, whether the body produced by the decomposition be soluble or not. What, therefore, is termed decomposing affinity, might be rejected. The reddening of tincture of litmus is an action of acids depending entirely on the presence of water. There are chemical compounds, which are formed entirely in consequence of the heat evolved by trituration."

In the combination of bodies, caloric is generally evolved, and the capacity of compounds for heat is consequently regarded as less than that of their components. Gay Lussac* has stated several cases, in which the compounds retain more heat than the components. Thus, a saturated solution of nitrate of ammonia, at the temperature of 16°,3, (centigrade,) and the density of 1,302, being mixed with water, in the proportion of 44·05 to 33·76; the temperature of the mixture fell to 4° 95, and its density was found at the temperature of 16° 3, to be 1·159, whilst the mean density is only 1·151. The chlorine of azote, discovered by M. Dulong, gave out, when decomposed and reduced into two simple bodies, chlorine and azote, both heat and light; and the same circumstance occurs, when iodine of azote is reduced into iodine and azote.

As one of the laws of combination, also, it has always been understood, that a metal must be in a state of oxydizement, in order to be united with an acid. According to M. Dulong, however, there are some exceptions to this rule: thus he conceives, when oxyd of zinc, or of lead, is combined with oxalic acid, and the compound dried, the hydrogen of the acid combines with oxygen of the oxide, the result is, a combination of *carbonic acid* and the *metal.** This is an opinion, however, in which we cannot concur, until we have either an opportunity of examining the circumstances on which it is founded ourselves, or it shall be confirmed by others.

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* *Annales de Chimie et de Physique,* tome i. p. 214.

M. Vogel has added some interesting facts to the few already known respecting the action of the solar rays, in regulating chemical combinations. He found, that ammonia and phosphorus, which do not act upon each other in the dark, when exposed to the light, combine intimately into the form of a black powder, and emit phosphoretted hydrogen gas. Phosphorus and potash are affected in nearly the same manner. The different rays, however, have different degrees of influence: the red, for instance, produces no effect on a solution of corrosive sublimate in ether; but a mutual decomposition is produced by the blue rays and a full light.† We, several years ago, remarked the effect of light, in altering the characters of resinous and other vegetable powders.

ANIMAL CHEMISTRY.

THE importance of this branch of chemical science to the physiologist, is too obvious to require any particular exposition. We have already noticed Dr. Marcet's experiments on the *Chyle* and *Chyme*;‡ and will, now, merely add some of the physical properties of these secretions. *Chyle* is a semitransparent, inodorous fluid, with a very slight milky hue, like diluted whey. It contains a coagulum, resembling the albumen of the egg, but of a pale pink hue. Chyle from animal food putrifies in a few days, whilst that from vegetable diet remains for a week before it begins to putrify: and in both the coagulum is more apt to suffer decomposition than the serous part. Chyle from animal food contains about seven per cent. of solid matter; whereas vegetable chyle yields seldom more than five per cent. *Chyme* is neither acid nor alkaline. When kept, it putrifies in ten or twelve days; and, when evaporated to dryness, leaves nearly one fifth of its weight of solid matter. It contains albumen; and, when burnt, one thousand parts yields twelve of charcoal, which also contains iron, lime, and a muriate of soda.

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* *Report of the Royal Institute of France;* vide *Philosophical Magazine*, vol. xlvii. p. 390.

† *Cuvier's Report on the Class of Mathematical and Physical Sciences of the Royal Institute of France*, for 1815.

‡ *Vide Medico-Chirurg. Trans.* vol. vi.—*Repository*, vol. v. p. 318.

The colour of *blood* is generally supposed to depend on iron; but some experiments by Vauquelin* have demonstrated the erroneous nature of this opinion, and proved, "that a colouring matter may be drawn from the blood, in which the most delicate test cannot discover the smallest trace of iron." The blood, according to Vauquelin's experiments, is composed of—1. albumen; 2. fibrine; 3. colouring matter; 4. a fat and mild oil; the constant presence of which, however, in the blood of man, requires the authority of further experiments.

Epidermis.—It has been supposed, with much probability, that this part of the animal frame is formed from the lymph contained in the lymphatics that pass through the skin. Mr. Hatchett regards that portion of it which is found on the human foot as indurated albumen, an opinion which is confirmed by the following analysis of it, by Professor John.† One hundred parts yielded:

Indurated albumen	93 to 95
Mucus, with a trace of animal (gelatinous?) matter	5
Lactic acid . . . . Lactate of potash . . Phosphate of potash . Muriate of potash . . Sulphate of lime . . Ammoniacal salt . . Phosphate of lime . . Manganese and iron .	1
Soft fat	0·05

The epidermis from the arm of a woman afflicted with herpes, was found, when treated in the same manner, to contain:—

Indurated albumen	92 to 93
Mucus becoming insoluble by evaporation; gelatinous mucus precipitated by galls . .	6 to 7
Lactic acid, and the above stated salts, (no manganese)	1
Soft fat, which remained soluble in diluted alcohol, but separated from concentrated alcohol by cooling	¾ to 1

M. Alibert found uncombined phosphoric acid in the scales, in an epidermis of a person labouring under herpes: in lepra he found only carbonate of lime.

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* *Annales de Chemie et de Physique*, tom. i. p. 9.

† *Schweigger's Journal*, vol. xiv. p. 302. *Annals of Philosophy*, vol. vii. p. 421.

Nails.—These, Professor John found to exhibit nearly the same components as the epidermis; except that the insoluble portion possesses the properties of indurated albumen.

Horns of black cattle have been examined by the same indefatigable experimenter, and found to consist of the following components:—

Indurated albumen, possessing much of the characters of mucus	90
Gelatinous mucus, with an animal matter thrown down by nutgalls	8
Lactic acid, and lactate of potash	1
Sulphate, muriate, and phosphate of potash	
Phosphate of lime	
Trace of oxide of iron	
Ammoniacal salt	
Fat, about	1

A peculiar volatile substance, which thickens more rapidly than volatile oil, and has the smell of horn.* The *hoof of the horse* possesses all the characters of horn.

NATURAL HISTORY.

M. T. Lauder Dick† has verified an observation of Gmelin, regarding the *stickleback* or Gasterosteus *aculeatus*. These small fish, after having deposited or impregnated their ova, begin to swell out on their sides in two parts; so as to assume, if the head and shoulders were taken off, the figure of a fiddle. This is owing to worms, the tænia solida, which are found lying immediately beneath the peritoneum. "The tænia appeared," on opening the fish, "to lie with their heads towards each other in the centre, and having their extremities folded or rolled up in the anterior and posterior regions of the cavity, so as to form the double protuberance, so distinctly visible in the external form of the fish when alive." In time, the worms force their way out at the anus of the fish, which falls a victim to the disease.

Mr. Dick is of opinion, that as this disease begins directly after the spawning, it is a provision of Nature, for preventing too great an accumulation of a particular species; but this opinion requires to be confirmed, by further observations made at different parts of the world, before it can be adopted.

Medicinal Leech.—Notwithstanding the importance of the leech, in a medical point of view, and the employment of it for the abstraction of blood, since very early periods; yet, it is remarkable, that so little had been done to investigate its natural history. Dr. James Rawlins Johnson has endeavoured to fill up this blank, and has executed the task in a very satisfactory manner.

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* *Annals of Philosophy.* vol. vii. p. 423. † *Ibid. p.* 106.

The first section of Dr. Johnson's work,* traces the medical history of the leech. Themison† was the first who wrote on the utility of the leech; but it was afterwards noticed by Antyllus, Menemachus, Pliny, Aretæus, and a host of writers, through whose works its medical history is traced to the present period. In applying leeches, the ancients frequently first punctured the skin with a lancet; and when the leech was in the act of suction, snipped off the tail, by which means a smaller number of leeches were required, as the blood flowed from the cut extremity.

In the second section, on "the Natural History of the Leech," Dr. Johnson first enumerates the species and varieties of hirudo, described by authors, and gives the systematic characters of the genus and each species, according to the place it holds in it. The H. *medicinales* are common throughout Europe: their colour varies according to the soil on which they are found; thus, at *Black River*, near Glastonbury, they are black; at *Cook's Corner*, of a reddish cast, from the red peat; and at Auler Moor, yellow, from the clay; but whatever this change may be, they still offer spots or lines, which may be considered permanent: these are, "supra lineis flavis sex, intermediis nigro arcuatis, subtus cinerea nigro maculata." The food of the medicinal leech is the blood of fish, frogs, &c.; but Dr. Johnson found that it does not touch the earth worm, nor the larva of the aquatic insects, nor take any solid food. Several interesting observations are given, regarding the habits of the horse-leech, which preys upon other leeches, and sometimes upon its own species. The leech is not reproductive when cut in pieces, as has been often supposed; its copulation is similar to that of the garden snail, the animal being hermaphrodite. Dr. Johnson is of opinion it is oviparous; an opinion which is consonant to the observations of those who have had the best opportunities of noticing the animal. Several instances are detailed of its extreme tenacity of life; and the results of some curious experiments given of the effects of various gases on it.

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* *A Treatise on the Medicinal Leech; including its Medical and Natural History, with a Description of its Anatomical Structure; also, Remarks upon the Diseases, Preservation, and Management of Leeches.* By James Rawlins Johnson, M. D. F. L. S. &c. 8vo. pp. 147, plates. Lond. 1816.

† He flourished in the first years of the Christian era.

The third section relates to "the anatomical structure of the leech;"—its external structure, the organs of the senses, and the respiratory organs, with its internal structure, are successively described. We regret that our limits prevent us from entering upon this part of the subject, which does not admit of much abridgment. The diseases to which leeches are subject, are then detailed; and the mode of preserving them pointed out. Extremes of heat and cold must be avoided, and the water changed once in eight days. When kept in large quantities, the vessel should have a false perforated bottom, and turf placed under it. In applying the leech, when it does not evince a disposition to bite, puncturing the part with a lancet, offers the only chance of success. When the leech drops off, instead of applying salt, which generally destroys the animal, a little vinegar poured on the head will make it disgorge the blood, and it will bite almost immediately afterwards.

BOTANY.

We cannot refrain from taking this opportunity of expressing our gratification of the attention that has lately been bestowed upon this branch of natural history, which is no longer to be regarded as a bare catalogue of names; but as a Science, embracing the consideration of whatever is connected with the vegetable economy.

The first and most important work which we have to notice on this subject, is the "*Description of the New Genera and Species of Plants,*"[*] discovered in the travels of the celebrated Humboldt and his friend Amatus Bonpland. During the five years these adventurous travellers were in South America, they collected 5,800 species of plants, of which 5,500 are phanerogamous,[†] and 300 unknown before to botanists. This number will appear very considerable, when it is remarked that the whole of the phanerogamous plants now known to grow within the tropics, in South America, do not exceed 13,000 species.

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[*] *Nova Genera et Species Plantarum, quas in peregrinatione ad plagam æquinoctialem orbis novi collegerunt, descripserunt partim adumbraverunt Amat. Bonpland et Alexander de Humboldt. Ex schedis autographis Amati Bonpland in ordinem digessit.* Carol. Sigismund Kunth, &c. tomus primus, 4to. Paris, 1815.

[†] The phanerogamous plants are those in which the sexual organs are conspicuous.

The most interesting part of the work is the observations of M. Humboldt on the geographical distribution of plants. We refer our readers to a very masterly analysis of it in the Annals of Philosophy, (vol. vii. p. 373.) The species of plants at present known, amount to 44,000, of which 6000 are cryptogamic, and 38,000 phanerogamic. The following is the distribution of the latter, according to Humboldt:

Europe, ...	7,000
Temperate regions of Asia, . . .	1,500
Asia within the tropics and islands, .	4,500
Africa, ...	3,000
Both temperate regions of America, .	4,000
America between the tropics, . .	13,000
New Holland, and islands in the Pacific,	5,000
	38,000

The following table exhibits the proportion of the different families of plants, observed by our travellers in South America, within the tropics :—

	Species.		*Species.*
Cyperoideæ, . .	68 or $\frac{1}{57}$	Compositæ,	600 or $\frac{1}{6}$
Gramineæ, .	256 or $\frac{1}{15}$	Umbelliferæ,	30 or $\frac{1}{129}$
Junceæ, . . .	9 or $\frac{1}{436}$	Cruciferæ,	19 or $\frac{1}{404}$
Three preceding families,	333 or $\frac{1}{11}$	Malvaceæ,	80 or $\frac{1}{47}$
Labiatæ, . .	95 or $\frac{1}{46}$	Leguminosæ,	314 or $\frac{1}{14}$

There are no firs on the mountains of South America, between the tropics ; nor are there such associations of the same species of plants as exist in the temperate zones : but in the woods a great variety of trees are found equally mixed.

Many curious and important tables, illustrative of the mean temperature of the same latitudes in America and the Old Continent, are given ; and also a catalogue of the distribution of plants within the tropics in South America, according to the height of the different places above the level of the sea, which is admirably illustrated by a plate exhibiting the distribution at different heights in the torrid, the temperate, and the frigid zones. The work is, altogether, one of the highest interest, and we have only to regret that it is not more within the pecuniary reach of the great majority of scientific readers.

The cause of the upright growth of plants, has long been a subject of controversy amongst botanists. Mr. Campbell, of

Carbrook, in a paper read at the Wernerian Natural History Society, last February,* has proposed a theory founded on the law of resisted attraction, by which he regards all ascents from the centre to be regulated. Evaporation, which is always present in vegetation, and the buoyancy produced by the formation of gases, afford, according to Mr. Campbell's ideas, a field for the operation of this law, which he regards as the chief agency in the upright growth of vegetables. Although there is much ingenuity in this explanation, yet, it does not accord with our ideas on the subject. Too much, in all the theories that have appeared, is attributed to mechanical agency, and the fact of plants being living bodies too much overlooked. It does not appear to us more extraordinary that living plants assume an upright position, than that animals walk on their feet instead of rolling on their backs.

M. Delabillardiere is giving an account of the plants he found in his expedition with Entrecasteaux. The genus *lemna* has been examined by the Baron de Beauvois, who has been able to collect ripe grains of the plant, and make them germinate. He has confirmed the opinion that the flower is hermaphrodite; and, what is very remarkable, the parts considered as the plume and radicle separate from the first leaf they produce, and leave it alone to push out roots and other leaves. M. de Laval has illustrated the propagation of the confervæ: having found it to depend on the concentration of the green matter contained between two cells into an isolate globule which issues from the plant at a certain time, and fixes itself on the first body it meets with in its fall; when, after having thrown out some filaments in order to fix itself, it developes a long series of cells. M. Kuhnt, a Prussian botanist, has undertaken a new classification of the *gramina*.

In this section we have only further to notice, that the Rev. Mr. Keith has published a New System of Physiological Botany, an analysis of which we propose giving to our readers at an early opportunity; and that the Linnean Society has published the eleventh volume of their Transactions.

MATERIA MEDICA AND PHARMACY.

On this subject we have scarcely any observations to communicate. *Decandolle* has published a second edition of his work, on the Medical Properties of Plants.* As many of our readers may not be acquainted with the work at all, we translate the conclusions which the author draws as the results of his examination of the individual genera.

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* *Annals of Philosophy*, vol. vii. p. 443.

"1. The same parts, or the corresponding juices of plants of the same genus, possesses similar medical properties.

"2. The same parts, or the corresponding juices of plants of the same natural family, enjoy similar medical properties.

"3. The following circumstances may produce exceptions to these rules: when, for instance, there is—*a.* A marked difference, although not noticed in botanical works, between the species of a genus and the genera of a natural family. *b.* A false comparison between the organs of analogous plants. *c.* An accidental and transient state, in which some vegetables are found at the time, when they are usually employed. *d.* Unequal mixtures of different chemical principles, really common to all the analogous plants. *e.* Differences in the mode of extracting or preparing them, by which their medical properties are modified. *f.* The ascribing too much importance to accidental properties. *g.* The difficulty of accurately comparing the modus operandi of different medicines. *h.* The difficulty of accurately comparing that action, owing to the various methods of applying medicines to the human body.

"4. Analogy leads to the belief, that the families to which apparently insuperable exceptions apply, in the present state of medical science, will be freed from these when medicine, chemistry, and botany, have arrived nearer to perfection."

This last conclusion is drawn from what has been done since the first edition of his work was published, ten years ago.

Colchicum autumnale.—Sir Everard Home, in a paper read to the Royal Society, has endeavoured to prove, that the beneficial effects of this medicine in gout are produced through the medium of the circulation into which it enters. It lowers the impetus of the pulse, ten or twenty beats, in about twelve hours after it is taken; which Sir Everard attributes to its then reaching the circulation; as the same effect is produced when the infusion of the medicine is injected into the veins. On injecting 160 drops of it into the jugular vein of a dog, the animal lost all power of motion, the breathing became impeded, the pulse at first very slow, but afterwards extremely quick; purging and vomiting supervened, and in five hours he died. On dissection, the viscera were found inflamed.

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* *Essai sur les Propriûtés Médicales des Plantes, comparées avec leurs formes exterieures et leur classification naturelle.* Par M. A. P. Decandolle, &c. Paris, 1816.

We have already noticed Dr. Peters, of Anclam, commendations of sedum *acre*, as a remedy for epilepsy;* M. Chaumeton's observations on the insufficiency of the Calaguala, or roots of the aspidium *coriaceum*;† and the observations of several German physicians, on the antispasmodic effects of bismuth, in cases of spasmodic affections of the intestinal canal, accompanied with much irritation.‡ We have also noticed the idea which has been suggested in Germany, that the narcotic effect of opium depends on the presence of prussic acid.§ How far the development of this powerful principle, in the preparation which has been lately introduced to the profession, under the name of *decomposed opium*,‖ is capable of accounting for the sedative effects of that preparation, we will not venture to assert; but, certainly it is one of very extraordinary powers, and a more manageable form of exhibiting opium, than any other with which we are acquainted. It does not appear to exert a stimulant action, previously to producing its sedative effect, as is the case with opium in any other form; but allays nervous irritations, and induces sleep without, in any manner, affecting the head.

THEORY AND PRACTICE OF MEDICINE.

FEVER, as the disease which is of most frequent occurrence and the most difficult of cure, and as being more or less symptomatic of every other disease, has always particularly excited the attention of the medical philosopher. Since the period of our last Report, no work has appeared in England directly on this subject. In France, M. I. F. Hernandez, in a treatise which he has intitled "Essai sur le Typhus,"¶ has entered pretty fully into an examination of malignant, putrid, bilious, and mucous (muqueuses) fevers, the yellow fever, and the plague. The first part of the work is purely theoretical. The author examines what changes the living fluids are capable of undergoing, and points out the erroneousness of the opinion, which supposes, that any state approaching to putridity can take place in the blood whilst it circulates in the vessels; or that there is what has been termed a bilious or a mucous degeneracy of the blood—his chief object, indeed, is to combat the humoral doctrines. In the second part, he treats of the classification of fevers; in the third, of the complications which typhus presents; and in the fourth, of those fevers which degenerate into typhus, as inflammatory and intermittent fevers. The fifth part is dedicated to the consideration of the contagious nature of typhus; comprehending plague, yellow fever, and the other species of the genus: and the sixth, of the medical treatment of the disease. The work is well put together; but the views of the author possess little of novelty.

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* *Repository*, vol. v. p. 175.

† *Journal de Pharmacie, Mai* 1816: and *Repository*, vol v. p. 526.

‡ *Repository*, vol v. p. 438. § *Ibid.* p. 353. ‖ *Ibid.*

¶ *Essai sur le Typhus, ou sur les Fièvres dites malignes, putrides, bilieuses, muqueuses, jaune, la peste, &c.* par I. F. Hernandez, Chevalier de la Legion d'Honneur, &c. 8vo. pp. 479. Paris, 1816.

No part of medicine has made more rapid advances than that which relates to female diseases. *M. Nanche* has just sent forth a very respectable work on this subject,* embracing the diseases connected with the uterine organs.

Another case of the successful effect of *evacuants* and *refrigerants*, in hydrocephalus internus, is added, by Dr. Bateman,† to the number we have been fortunate enough to record. The disease had gained considerable progress, the symptoms (the patient was a boy eight years of age) being "rapid pulse, partially flushed countenance, dilated pupils, and extreme pain in the head, so that he screamed upon being moved, or even upon the apprehension of being moved." He was also nearly blind, and strabismus was perceptible. Calomel and jalap were freely exhibited, and the head shaved and washed with water and ether. Leeches and mercurial frictions were employed; but, as Dr. B. observes, the relief was "obviously to be ascribed to the evacuant and refrigerant remedies."

A work of some interest has appeared on the Vesaniæ, by M. J. R. Jacquelin-Dubuisson.‡ On the same subject, Dr. J. Reid has published a volume of Essays,‖ which, although containing many important truths, yet, are not offered by the author as possessing much novelty. Dr. Reid, indeed, in his advertisement, acknowledges, that many passages in them have been taken, without much alteration, from the Medical Reports, which, after Dr. Willan had relinquished the task, were taken up by Dr. Reid, and communicated to the Old Monthly Magazine. As having some affinity with these subjects, we have to notice a little Treatise on Incubus, by Mr. John Waller.* The author, who appears to have written on this complaint from his own personal experience of its effects, observes, that it may occur in a very profound sleep, without breaking or disturbing that sleep, further than to produce a dream, the memory of which will remain, when awake, in the same manner as that of any other impressive dream: but it happens more frequently, when the patient enjoys an imperfect sleep. It has almost constantly occurred to the author, that the power of volition has been partially recovered for some time before the paroxysms cease. From these and other observations, Mr. Waller concludes, "that the nature of incubus is essentially different from that of either sleeping or waking, and is an absolute state of disease which may occur during sleep, in a slight degree, without breaking that sleep; that it consists in a suspension of the power of volition, very different to what takes place in ordinary sleep, because it is accompanied with a consciousness of that suspension, and a strong desire to recover this power." As the disease arises from a defect in the digestive organs, whatever tends to correct these will facilitate the cure; the bowels should be kept open, and small doses of carbonate of ammonia exhibited with bitters or aromatics and purgative.

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* *Des Maladies de l'Uterus ou de la Matrice*, par M. Nanche, Medecin consultant de l'Institution Royale des Jeunes Aveugles, &c. 8vo. pp. 461. Paris, 1816.

† *Edin. Journal*, vol. xii. p. 119.

‡ *Des Vesanics, ou Maladies mentales*, par J. R. Jacquelin-Dubuisson, Docteur en Medecine, &c. 8vo. pp. 303. London, 1816.

‖ *Essays on Insanity, Hypochondriasis, and other nervous Affections*, by J. Reid, M. D. 8vo. London, 1816.

Dr. Meglin has published a small collection of cases and remarks on *tic douloureux*,† chiefly with the view of recommending the employment of hyosciamus *niger*, sulphate of zinc and wild valerian root, which he regards as nearly specific in this spasmodic affection. The two former remedies have been already tried in this country, with very little effect.

In concluding these few observations on the practice of medicine, we shall only further mention the paper of our correspondents, Dr. Fraser on cynanche laryngea;[*] that of Mr. Bedingfield on the ulceration of the pharynx and larynx;[†] and of Mr. Stevenson on pemphigus diutinus;[‡] as particularly worthy of attention.

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* *A Treatise on the Incubus, or Night-Mare, disturbed Sleep, terrific Dreams, and nocturnal Visions, &c.* by John Waller, Surgeon of the Royal Navy. 12mo. pp. 115. London, 1816.

† *Recherches et Observations sur la Neuralgie faciale, ou Tic Douloureux de la Face*, par M. Meglin, Docteur en Medecine, &c. 8vo. pp. 121. Strasburg, 1816.

On Medical Jurisprudence, Orfila's work embraces, in itself, every thing that has been done; but as we have given so ample an analysis of it, no particular mention of its contents are required in this place. In this country, Dr. Male has published an Epitome af Juridical or Forensic Medicine; but it is too deficient in details to excite much interest, particularly since the work of Orfila has received an English dress.

Observations on the treatment of Varicose veins of the legs. By B. C. Brodie, Esq. F. R. S. *Assistant Surgeon to St. George's Hospital, and Lecturer on Surgery.*

[From the Medico-Chirurgical Transactions.]

IT seems to be established by the experience of modern surgery, that a mechanical injury inflicted on the trunk of one of the larger veins, is liable to be followed by inflammation of its internal membrane, and a fever of a very serious nature; and the occasional occurrence of these symptoms after the ligature, or even the simple division of the vena saphæna, has occasioned surgeons to be cautious in performing these operations for the relief of a varicose state of its branches in the leg.

But are the same ill effects likely to take place if a similar operation be performed on the branches themselves? Reflecting on the following circumstances, I was induced to answer this question in the negative. Varicose veins of the legs are so frequent in persons of the inferiour order, (at least in this metropolis) that it cannot be otherwise, than that a considerable proportion of those who apply at the hospitals on account of wounds of the legs, must labour under this disease; yet I do not recollect an instance of venous inflammation following such an accident; neither has such an instance occurred among a great number of cases which have come under my observation, in which the varicose veins of the rectum forming piles, have been removed by excision or by ligature.

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[*] *Repository*, vol. v. p. 453. [†] *Ibid.* p. 194. [‡] *Ibid.* p. 290.

It was recommended by Celsus to destroy varices of the leg, by the cautery, or by extirpating them with the knife; and the same operations have been performed (though but seldom) by some more modern surgeons. The consideration of the circumstances, which have been just stated, led me to venture on the adoption of a practice somewhat corresponding (though not exactly similar) to that of Celsus. Finding that it was attended with benefit to my patients, and that no ill consequences ensued, I did not hesitate to repeat the experiment in a considerable number of cases. It is the result of my observations on this subject, which I have now the honour of laying before this society; not in the belief that I am communicating surgical facts of the first importance, but hoping, nevertheless, that what I have to mention, will be found not altogether undeserving the notice of the profession.

Where the whole of the veins of the leg are in a state of morbid dilatation, and the distress produced by the disease is not referred to any particular part; there seem to be no reasonable expectations of benefit, except from the uniform pressure of a well applied bandage. But not unfrequently, we find an ulcer which is irritable and difficult to heal on account of its connexion with some varicose vessels; or, without being accompanied by an ulcer, there is a varix in one part of the leg, painful, and perhaps liable to bleed, while the veins in other parts are nearly in a natural state, or, at any rate, are not the source of particular uneasiness. In some of these cases I formerly applied the caustic potash, so as to make a slough of the skin and veins beneath it; but I found the relief which the patient experienced from the cure of the varix to afford but an inadequate compensation for the pain, to which he was subjected by the use of the caustic, and the inconvenience arising from the tedious healing of the ulcer, which remained after the separation of the slough.

In other cases, I made an incision with a scalpel, through the varix and skin over it. This destroyed the varix as completely as it was destroyed by the caustic, and I found it to be preferable to the use of the caustic, as the operation occasioned less pain, and as (in consequence of there being no loss of substance) the wound was cicatrized in a much shorter space of time. I employed the operation, such as I have described it, with advantage in several instances; but some months ago, I made an improvement in the method of performing it, by which it is much simplified; rendered less formidable, not only in appearance, but also in reality; and followed by an equally certain, but more speedy cure.

It is evident, that the extensive division of the skin over a varix can be attended with no advantage. On the contrary, there must be a disadvantage in it, as a certain time will necessarily be required for the cicatrization of the external wound. The improvement, to which I allude, consists in this: that the varicose vessels are completely divided, while the skin over them is preserved entire, with the exception of a moderate puncture, which is necessary for the introduction of the instrument with which the incision of the veins is effected. Thus, the wound of the internal parts is placed under the most favorable circumstances for being healed, and the patient avoids the more tedious process, which is necessary for the cicatrization of a wound in the skin above.

For this operation, I have generally employed a narrow, sharp-pointed bistoury, slightly curved, with its cutting edge on the convex side, such as is delineated in the annexed drawing. Having ascertained the precise situation of the vein, or cluster of veins, from which the distress of the patient appears principally to arise, I introduce the point of the bistoury through the skin on one side of the varix, and pass it on between the skin and the vein, with one of the flat surfaces turned forwards, and the other backwards, until it reaches the opposite side. I then turn the cutting edge of the bistoury backwards, and in withdrawing the instrument the division of the varix is effected. The patient experiences pain, which is occasionally severe, but subsides in the course of a short time. There is always hæmorrhage, which would be often profuse if neglected, but which is readily stopped by a moderate pressure made by means of a compress and bandage, carefully applied. The same pressure which is necessary for the suppression of the hæmorrhage, is useful, as it keeps the divided surfaces in contact, so that they may have the opportunity of uniting by the first intention. With a view to the more certain attainment of this last object, care should be taken not to divide, very extensively, the soft parts below the varicose vessels. If the edge of the bistoury be sufficiently sharp, a moderate pressure will answer the purpose required; and if the pressure be considerable, a wound much deeper than is necessary will be the consequence. With the same view, the patient should be kept, for the first four or five days in bed, in a state of perfect quietude, and when the bandage which has been applied is removed, this should be done with the greatest caution, lest any union which may have taken place be destroyed, in consequence of the uniting substance not having as yet acquired the due firmness of texture. By attending to these circumstances, an immediate cure of the wound is generally effected: where it is otherwise, no very considerable time is necessary for it to become healed by the process of granulation.

In every case, in which this operation has been hitherto performed, I have found it to be followed by the obliteration of the varix, and, indeed, it is difficult to conceive how it should fail in producing this effect. Sometimes no vestige of the divided veins has been to be distinguished afterwards; at other times they have remained for a certain period full of solid coagulum, which has gradually been absorbed. This difference probably depends on the different degrees of pressure made by the compress and bandage, and on the circumstance of the pressure being confined to the line of the incision; or of its being extended over the whole cluster of dilated veins. If there have been veins in a varicose state below those, which have been divided, and communicating with them, these have become contracted in size, and usually have presented no appearance of disease afterwards. The good effects of the operation have, however, been most apparent in cases of varicose ulcer. In most instances the pain in the ulcer has ceased immediately. When the ulcer has been of a moderate size, it has sometimes been found perfectly healed in a few days, on the first removal of the bandages;—when it has been of a large size, it has begun to heal rapidly, although it had made perhaps little or no progress towards amendment under the treatment which had been previously adopted. Where there has been a varix below the ulcer, the division of it has been attended with more relief than that of the varix above the ulcer in other cases.

Inflammation of the coats of the veins has not occurred in any of the cases, in which I have hitherto adopted this method of treatment. I have already observed, that there are some reasons for believing that the venous branches are less liable to be inflamed, in consequence of mechanical injury, than the trunks, in which they terminate. But, perhaps, something is also to be attributed to the integuments over the wounded vein being left entire. It is not unreasonable to suppose that a vein under these circumstances is not equally disposed to take on the action of inflammation with one, which after its division is exposed to the contact of the air, or other extraneous substance. The difference of the injury corresponds to that which exists between a simple and compound fracture, and it seems probable that there should be, to a certain degree, a corresponding difference in the effects which are produced.

In two or three cases inflammation of the adipose and cellular membrane has taken place, producing pain and tenderness of the limb, and a slight degree of fever; but this has speedily subsided with only this ill consequence, that the wound has failed in becoming united by the first intention, and that the healing of it has been effected afterwards, by the more tedious process of suppuration and granulation. The treatment, in these cases, is very simple. Cold lotions may be applied in the first instance, for the purpose of moderating the inflammation. When suppuration has begun, the parts may be fomented and poulticed, and the ultimate cure of the small abscess which remains may be promoted by a moderate pressure made with strips of linen spread with soap cerate, applied circularly round the limb.

In two instances the operation has been followed by an attack of erysipelas; but this must be regarded as an accidental occurrence, there having been at the same time several other patients in the hospital, labouring under this disease.

Having made these general obsevations, I shall not intrude on the patience of the society, by giving a detailed account of the whole of the cases from which they are drawn. From those of which I have preserved notes, I have, however, selected the four following, the relation of which, will be sufficient to illustrate the remarks which have been made, and to explain the circumstances connected with this subject, which principally demand the attention of the surgeon.

CASE I.

Mary Narraway, 45 years of age, was first admitted into St. George's Hospital, on the 12th of October, 1814; on account of pain, swelling, and ulcers of the legs, with many large clusters of varicose veins.

October 15. Two clusters of varicose veins which were larger and occasioned more uneasiness than the rest, situated on the posterior part of the calf of the right leg, were divided with the skin over them, by two incisions. There was considerable hæmorrhage, which was stopped [illegible] the application of a bandage.

Some inflammation of the skin and cellular membrane took place after the operation, for which she was bled. The wounds made by the operation were not completely healed till after some weeks. She was then affected by an inflammation of the eyes, on account of which she remained in the hospital till the 17th of January. At this time there was no vestige of the clusters of veins which had been divided, and the veins of the right leg generally were much diminished in size.

She was re-admitted into the hospital on the 31st of January, 1816.

She said that her right leg had been completely relieved by the operation, and that she had no uneasiness in it until three weeks ago; when a small ulcer took place on the inside, a little above the inner ankle.

At the time of her re-admission there was a very painful ulcer of the size of a sixpence, on the inside of the right leg, with some varicose veins above and below the ulcer. The whole of the posterior and outer part of the leg, from the cicatrix downwards, presented no appearance of varicose vessels, and was free from pain; but the pain in the ulcer on the inside was such that she could scarcely bear to stand in the upright position.

In the left leg there were the same clusters of varicose veins which had existed when she was in the hospital formerly, but somewhat increased in size, and a quantity of purple discoloured skin on the inside. She said that she had for a considerable time laboured under a painful varicose ulcer of this leg, which had at last healed under the use of adhesive plaster.

February 10. The cluster of varicose veins above, as well as that below the ulcer of the right leg, were divided in the manner described in the former part of this communication; the skin over them being left entire. The division of the veins occasioned considerable pain, which subsided in about an hour and a half.

February 14. On removing the bandage the ulcer was found perfectly healed. She was free from pain. The wounds made by the operation had healed by the first intention.

March 4. There was still a slight tenderness in the situation of the wounds; otherwise she was free from all uneasiness. There were no remains of the varices, which had been divided.

Case II.

Anne Sadler, [illegible] years of age, was admitted into St. George's Hospital on the 21st of August, 1815, on account of varicose veins of the left leg, with a varicose ulcer of the inner ankle. She complained of great pain on the inside of the leg, which tormented her day and night.

On the 17th of September, two large varicose veins on the inside of the leg, were divided by means of the bistoury introduced obliquely under the skin. One of these veins was situated about an inch and a half above the inner ankle, and the other, about four inches higher, on the inside of the leg.

The wounds made by the operation, inflamed so as to suppurate, and afterwards healed by granulation.

In the beginning of October, the wounds were completely cicatrized; the ulcer of the inner ankle was skinned over; no vestiges were perceptible of the veins which had been divided. She was free from all uneasiness, except a very slight degree of tenderness in the situation of the wounds made in the operation.

October 9. She was discharged from the hospital as cured.

Case III.

William Haines, 52 years of age, was admitted into St. George's Hospital on the 6th of January, 1816.

He had two varicose ulcers on the inner ankle of the left leg, one of the size of a shilling, the other smaller. These ulcers had existed for two years, and were exceedingly painful.—There was a considerable varicose vein below the ulcer, and another of a larger size above; extending upwards from the ulcers, to join the vena saphæna major. There were two clusters of varicose veins on the calf of the same leg, connected with both the saphæna major and saphæna minor; one of them of a very large size.

The veins of the other leg, were varicose in many parts, but there were no ulcers.

January 13. The varicose vein below the ulcers of the left leg was divided in the manner already explained. The large varicose vein extending upwards from the ulcers was divided also, about three inches above the ankle.

The operation occasioned considerable pain, which lasted through the whole night.

January 14. He was more free from pain than he had been for a long time before.

January 18. The compresses and bandages applied at the time of the operation, were removed. The ulcers were healed. The wounds made by the bistoury had united by the first intention.

January 19. The larger of the two varicose clusters on the calf of the leg was divided.

January 24. The bandages were removed. The wound made by the last operation had also united by the first intention. The leg was bound up in strips of linen spread with soap plaster, and a bandage over them.

February 20. He left the hospital. At this time the veins of the right leg were in the same state as at the time of his admission. There were no remains of the veins which had been divided on the left leg: there was no vestige of the ulcers, and the discoloured skin round them had recovered its natural appearance. He was desired to continue the use of the bandage on both legs.

CASE IV.

Patrick Curley, 50 years of age, was admitted into St. George's Hospital on the 17th of January, 1816.

He had a varicose ulcer on each ankle of the left leg, not less than one inch and a half in diameter. There was an extensive cluster of varicose veins on the inside of the leg, above the ulcer of the inner ankle; and a smaller cluster on the outside, above the other ulcer. There was also a cluster of varicose veins on the calf of the leg. He had violent pain in both ulcers.

January 18. The varicose vessels on the inside of the leg were divided in three places, in the same manner as in the last case. The operation occasioned considerable pain, which lasted for four hours.

January 21. On removing the bandages, the incisions made in the operation were found to have healed by the first intention. The ulcers appeared more healthy. They were dressed with strips of adhesive plaster. He did not leave his bed until the 31st of January. He was now free from all uneasiness in the inside of the leg; but had a good deal of pain in the other ulcer.

February 20. The ulcer on the inside of the leg, below the divided veins, was almost completely healed. The ulcer on the outside of the leg was somewhat, but very little smaller. He complained of its being very painful, so as to disturb his rest at night. Three varicose veins were divided above, and one below the ulcer. The pain of the ulcer was immediately relieved, and he slept better on the following night, than for several nights previous.

February 24. He was unfortunately seized with erysipelas affecting the whole of the left leg, and attended with the usual constitutional symptoms. The erysipelas terminated in abscess, the matter of which it was found necessary to evacuate by three punctures in the foot and calf of the leg. When the erysipelas had subsided, the ulcer on the inside had been completely cicatrized for a considerable time: the ulcer on the outside was nearly cicatrized also. There were no evident remains of the divided varicose vessels.

From the result of the foregoing and of many other cases, I am induced to conclude, that the operation which has been described, may be frequently employed with great advantage to the patient. At the same time I wish to be understood as recommending the adoption of it, not indiscriminately, but with a due attention to the circumstances of each individual case. The cases for which it is fitted, are, not those, in which the veins of the leg generally are varicose, or in which the patient has little or no inconvenience from the complaint, but those in which there is considerable pain referred to a particular varix, or in which hæmorrhage is liable to take place from the giving way of the dilated vessels, or in which they occasion an irritable and obstinate varicose ulcer.

Further Observations on the Diseases which affect the Synovial Membranes of Joints. By B. C. Brodie, Esq. F.R.S. *assistant Surgeon to St. George's Hospital, &c.*

[From the Medico-Chirurgical Transactions.]

I.

DURING the last session I had the honour of presenting to the Society an account of several cases, which appeared to throw light on the pathological history and classification of the diseases of the human joints. In the present paper, I propose to communicate some brief practical observations on the symptoms, by which those diseases, which affect the synovial membranes, are to be distinguished, and on the treatment, which should be adopted for their relief. On the latter subject, indeed, what I have to offer may seem to have little or no claim to the merit of novelty. The effects of the various methods of treatment now employed by physicians and surgeons are, for the most part, of accidental discovery. The improvement of scientific pathology seldom leads to the invention of new remedies; but it enables us to understand better the application of those, which are already in use; to know the particular cases to which particular remedies are suited, and to distinguish the curable diseases, from those, in which a cure cannot be effected.

II. *On the causes and symptoms of inflammation of the Synovial Membrane.*

Inflammation of the synovial membranes may occur as a symptom of a constitutional disease, where the system is affected by rheumatism; where mercury has been exhibited improperly, or in large quantities, or where there is general debility from any other cause. But in these cases, the inflammation is seldom severe; it occasions an effusion of fluid into the joint, but rarely terminates in the extravasation of coagulable lymph, or thickening of the inflamed membrane. Sometimes it leaves one joint to attack another, or it suddenly subsides without another joint becoming affected.

At other times the inflammation occurs as a local affection produced by a sprain, the application of cold, or arising from no evident cause. It is here for the most part more severe, and of longer duration: it leaves the joint with its functions more or less impaired, and occasionally terminates in its total destruction. In itself it is a serious disease, but it is often confounded, under the general name of white swelling, with other diseases, still more serious. In some cases, it assumes the form of an acute, but in the greater number of instances, it has that of a chronic inflammation.*

In the former, there is pain and tenderness of the joint, and usually redness of the skin. The pain is severe; not referred to a particular spot, but to the whole joint. When the pain has existed for some time, swelling takes place. The period at which the swelling shews itself, varies from a few hours, to two or three days, from the commencement of the attack. The patient usually keeps the limb a little bent, and every attempt to bend or extend it further aggravates the pain. With these symptoms, there is inflammatory fever. In a few days the disease either subsides altogether, or assumes the chronic form: more frequently it has this form from the beginning.

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* It is to be observed, that the boundaries of acute and chronic inflammation in these, and in other cases, are not well defined. These terms accurately enough express the two extremes, but there are intermediate degrees of inflammation, of which it is difficult to decide, whether they should be considered as being of the acute or chronic kind. Language cannot supply names for all the varieties of morbid action which occur, but the surgeon will learn to distinguish them, and the corresponding varieties of treatment, and it is this nicer discrimination which forms a principal difference between the experienced and inexperienced practitioner.

Where there is chronic inflammation of the synovial membrane, the pain and tenderness are less, so that the patient is able to walk about, and often without experiencing any severe distress. There is no unusual redness of the skin, and little or no fever. The swelling begins soon after the commencement of the attack, but it increases less rapidly than where the inflammation is acute. These symptoms vary in degree at different periods, and generally are aggravated by exposure to cold or any unusual exertion.

In the first instance, the swelling of the joint arises entirely from a preternatural quantity of synovia being collected in its cavity. In the superficial joints, the fluid may be distincly felt to undulate, when pressure is made alternately by the two hands placed one on each side. When the inflammation has existed for some time, the fluid is less perceptible than before, in consequence of the synovial membrane having become thickened, or from the effusion of lymph on its inner or outer surface; and in many cases, when the disease has been of long standing, although the joint is much swollen, and symptoms of inflammation still exist, the fluid in its cavity is scarcely to be felt. As the swelling consists more of solid substance, so the natural mobility of the joint is in a greater degree impaired.

The form of the swelling deserves notice. It is not that of the articulating ends of the bones, and therefore it differs from the natural form of the joint. The swelling arises principally from the distended state of the synovial membrane, and hence its figure depends in great measure on the situation of the ligaments and tendons, which resist it in certain directions and allow it to take place in others. Thus, when the knee is affected, the swelling is principally observable on the anterior and lower part of the thigh, under the extensor muscles, where there is only a yielding cellular structure between those muscles and the bone. It is also often considerable in the spaces between the ligament of the patella and the lateral ligaments, the fluid collected in the cavity causing the fatty substance of the joint to protrude in this situation, where the resistance of the external parts is less than elsewhere. In the elbow the swelling is principally observable on the posterior part of the arm, above the olecranon and under the extensor muscles of the fore-arm; and in the ankle it shews itself on each side, in the space between the lateral ligaments and the tendons, which are situated on the anterior part. In like manner in other joints, the figure of the swelling, whether it arises from fluid, alone, or joined with solid substance, depends, in great measure, on the ligaments and tendons in the neighbourhood, and on the degree of resistance which they afford, and these circumstances, though apparently trifling, deserve our attention, as they enable us more readily to form our diagnosis.

In the hip and shoulder the disease occurs less frequently than in the superficial joints. The effused fluid cannot here be felt to undulate, but the swelling is perceptible through the muscles which cover it. When the hip is affected the pain is usually confined to the hip itself; sometimes it is referred to the knee also, as in cases where the cartilages of the hip are ulcerated. The following circumstances enable us to distinguish the two diseases from each other. In the former the pain is more severe in the first instance than afterwards; and there is swelling of the nates. In the latter the pain is trifling at first; becomes gradually worse, till it is at last exceedingly severe, and the nates are wasted and flattened instead of being swollen.

After the inflammation of the synovial membrane has subsided, the fluid is absorbed, and in some instances the joint regains its natural figure and mobility; but in the majority of cases stiffness and swelling remain. In the superficial joints the swelling has sometimes the form of the articulating ends of the bones, (that is, the natural form of the joint,) and we may suppose it in this case to arise from the thickened state of the synovial membrane. At other times it has the same peculiar form, which it possessed while the inflammation existed, and while fluid was contained in the joint, and we may suppose that it depends principally on the inner surface of the synovial membrane having a thick lining of coagulable lymph.

The chronic inflammation of the synovial membrane often continues for many months, and after having subsided is very liable to recur from slight causes. Thus a person has this disease in his knee; the inflammation is cured, but tumor and stiffness remain. Whenever he is exposed to cold, or exercises the limb in an unusual degree, and often without any evident reason, the pain returns and the swelling is augmented. Such cases are of frequent occurrence, and they form a large proportion of those which are known by the name of white swelling.

Long continued and neglected inflammation of the synovial membrane occasionally terminates in the formation of an abscess in the cavity of the joint, in ulceration of the cartilages, and in complete destruction of the articulating surfaces. In this last stage, if we wish to know whether the inflammation of the synovial membrane, or the ulceration of the cartilage, has been the primary disease, we must form our judgment, not from the present symptoms, but from the previous history of the case. It is indeed often difficult to procure a history, on the accuracy of which we can rely, particularly in hospital practice; but this is of less importance, as whatever the disease may have been in its origin, where it has proceeded so far as has been described, there is no difference with respect to the treatment; and in general, no remedy can be employed with any prospect of advantage, except the removal of the limb by amputation.

III. *On the Treatment of the Inflammation of the Synovial Membrane.*

In the acute form of the disease, leeches may be applied to the part; and in most instances it will be right to take blood from the arm. Warm fomentations produce better effects than cold lotions. Attention should be paid to the state of the bowels, and Dover's powder, or some other diaphoretic medicine, may be exhibited. Under this treatment the acute inflammation in general speedily subsides.

The chronic inflammation is relieved more slowly. In the first instance, the joint should be kept in a state of perfect rest. Blood should be taken from the part by means of leeches or cupping, and this may be followed by the application of a blister, large enough to include the greater part of the circumference of the joint. Under this treatment the pain is relieved, and in a few days the swelling, as far as it depends on the fluid collected in the cavity of the joint, is much diminished. Even where the tumor is solid, arising from the effusion of coagulable lymph, it will in a great degree subside, and sometimes be entirely dispersed, provided the lymph has not yet become organized. A single blister often produces marked good effects, but it is generally necessary to repeat both the blister and the blood-letting several times. The repeated application of blisters is more efficacious than a single blister kept open by the savine cerate, or by other means, for a considerable time. When the inflammation is in a great measure subdued, a moderate degree of exercise of the joint is rather beneficial than otherwise. Liniments, which irritate the skin, may be rubbed on twice or three times in the day. The following liniment is more stimulating than those in common use, and has appeared to me in most instances to have been productive of much better effects with respect to the disease.

Olei Olivæ ℥iss.
Acidi Sulphurici ℥ſs. M. fiat linimentum.

It may be used of this strength for the class of persons, who apply at an hospital for relief; but for persons of a higher class in society, in whom the cuticle is thinner, and the cutis more easily irritated, the proportion of the olive oil should be greater. The effect of this liniment is to excite some degree of inflammation of the skin; the cuticle becomes of a brown colour, and separates in thick, broad scales, and the inflammation of the internal parts is relieved, probably on the same principle as by a blister. The friction used in applying the liniment appears to be of service after the inflammation is nearly subdued, but if friction be employed in the first instance the disease is aggravated.

No other remedies seem to be productive of much benefit.

Issues and setons, which are useful in cases of ulceration of the cartilage, are of no service whatever in this disease.

Plasters of gum ammoniac, and others of a similar nature, are of little efficacy, while inflammation still exists; but afterwards they are of use in guarding the joint from the influence of the external cold, and preventing a relapse.

The swelling and stiffness that remain after the inflammation has subsided, if moderate in degree, may be relieved by the free exercise of the limb, and by friction. The mercurial ointment with camphor may be rubbed on the joint; or friction may be made by the hand with starch or other fine powder. The friction however should be employed with caution, as, when used too freely, it sometimes occasions a return of the inflammation. Whenever there is the slightest indication of this being the case, the friction should be omitted for a time, and leeches should be applied, and if the friction be resumed it should be employed in a less degree, and less frequently than before.

When the swelling and stiffness in consequence of the inflammation are very considerable, I have seldom known much, and I have never known entire relief produced by friction or by any other means. Here too the patient is more liable to a return of the inflammation, and hence friction must be employed with still greater caution than in other cases.

On the whole, I have not found so much good produced by friction, as from what I had heard of its effects, I had been led to expect; and I have known it, when used too freely, or too early in the disease, to delay rather than to expedite the cure. Friction appears to be more efficacious where the stiffness of a joint depends on a contracted state of the muscles or tendons of the limb, and on these being glued to each other and to the surrounding parts, than where it is the consequence of disease in the joint itself.

I have, in several instances, tried the effect of pumping hot water on a stiff joint, as recommended by Le Dran, and as now practised at some watering-places. The blow of a column of water falling from a height of several feet produces considerable friction, with which are combined the relaxing powers of heat and moisture. This practice is certainly productive of benefit; but the observations just made apply to this as well as to the other modes of producing friction.

IV. *On the Symptoms produced in those Cases, in which the Synovial Membrane has undergone a morbid Change of Structure.*

In my former communication respecting the diseases of the joints, I gave an account of several cases in which the synovial membrane had undergone a peculiar morbid alteration of structure. The observations which I then made have been fully confirmed by many similar cases, which have since come under my notice. This disease generally takes place in young persons under, or not much above, the age of puberty. I do not recollect more than one instance of it having occurred after the middle period of life. In general it can be traced to no evident cause; but occasionally it takes place as a consequence of repeated attacks of inflammation. In this respect it resembles other diseases of the same order. Inflammation of the lungs may produce tubercles, and inflammation of the breast may occasion the growth of a schirrous tumor. Where I have had an opportunity of examining the morbid appearances after amputation, I have always found the whole, or nearly the whole of the synovial membrane affected by the disease; but it is probable, that if the examinations were made at an earlier period, we should always find the morbid change originating in some one point. A patient was admitted into St. George's Hospital in whom this disease was beginning on the inside of the knee: from thence (in the course of three months) it gradually extended itself, in every direction, over the whole circumference of the joint. In a girl, who laboured under this affection of the knee, but who died in the hospital of another complaint, I found one half of the synovial membrane in a state of disease, and the other half retaining its natural structure and appearance.

In the origin of this disease there is a slight degree of stiffness and tumefaction, without pain, and producing only the most trifling inconvenience. These symptoms gradually increase ; at last the joint scarcely admits of the smallest motion, the stiffness being greater than where it is the consequence of simple inflammation. The form of the swelling bears some resemblance to that in cases of inflammation of the synovial membrane, but it is less regular. The swelling is soft and elastic, and gives to the hand a sensation as if it contained fluid. If only one hand be employed in making the examination, the deception may be complete, and the most experienced surgeon may be led to suppose that there is fluid in the joint, when there is none ; but if both hands be employed, one on each side, the absence of fluid is distinguished by the want of fluctuation.

The patient experiences little or no pain until abscesses begin to form, and the cartilages ulcerate ; and even then the pain is not so severe, as where the ulceration of the cartilages occurs as a primary disease, and the abscesses heal more readily, and discharge a smaller quantity of pus than in cases of this last description. At this period the patient becomes affected with hectic fever ; loses his flesh, and gradually sinks, unless the limb be removed by an operation.

The progress of this disease varies in different cases. In general one or two years elapse before it reaches its most advanced stage ; but sometimes the period is much longer ; and occasionally it becomes indolent, so that it remains during many months without any sensible alteration. In like manner tubercles of the lungs, or schirrus of the breast in some instances remain in an inactive state for several months, or even for some years.

The diagnosis of this disease is seldom difficult. The gradual progress of the enlargement and stiffness of the joint without pain ; and the soft elastic swelling without fluctuation, in most instances enable us to distinguish it, not only from inflammation of the synovial membrane, but also from the other morbid affections to which the joints are liable.

V. *On the Treatment of Cases, in which the Synovial Membrane has undergone a morbid Alteration of Structure.*

Where there is swelling and hardness following inflammation, the substance which has been effused may be absorbed, and the swelling and hardness may disappear, but I know of no instance in which an organ having completely lost its natural structure is capable of having that structure restored. Physicians and surgeons have been employed during successive ages in endeavouring to discover a cure for tubercles of the lungs, and cancer of the breast, and the result of their labours is only to prove that these diseases are incurable. Analogy therefore would not lead us to be sanguine as to the discovery of a remedy for this disease of the synovial membrane, and experience shews that it is equally incurable with other maladies of the same order. By means of rest and cold lotions, the progress of the disease may be somewhat checked, as the suppuration of tuberculated lungs may be retarded by occasional bleeding, and a milder climate; but ultimately the ulceration of the cartilages, the formation of abscesses in the cavity of the joint, and the consequent disturbance of the general health, render the amputation of the limb necessary in order to preserve the patient's life. At this period, therefore, the surgeon is called upon to recommend, and to urge an operation; but at an earlier period it is a matter of choice with the patient, whether he will live with the incumbrance of an useless limb, till the advanced stage of the disease renders its removal indispensable, or whether he will submit to the loss of it before the absolute necessity for losing it exists.

REVIEW.

Researches about Atmospherical Phenomena. By THOMAS FORSTER, F. L. S. *second edition, corrected and enlarged; with a series of engravings, illustrative of the modifications of the clouds, &c.*

ΕΣΤΙΝ. ΑΝΘΡΩΠΟΙΣ. ΑΝΕΜΩΝ. ΟΤΕ. ΠΛΕΙΣΤΑ. ΧΡΗΣΙΣ. ΕΣΤΙΝ.
Δ' ΟΥΡΑΝΙΩΝ. ΥΔΑΤΩΝ. ΟΜΒΡΙΩΝ. ΠΑΙΔΩΝ. ΝΕΦΕΛΑΣ.

London, &c. &c. &c. 8vo. 1815.

MANY of the most common appearances and events of nature have attracted but little of the attention and research of scientific men. Phenomena which rarely happen, and which afford only few opportunities for observation, are made the subjects of much curious speculation, and many ingenious hypotheses; while operations, which are continually going on within our view, are apt to be overlooked and neglected. Hence we may account for the little knowledge we yet possess on a variety of subjects, connected with meteorology. There is, perhaps, no branch of physics, unless we except magnetism, which has been cultivated with less zeal, and the labours in which have been rewarded with less success. This fact is not a little remarkable, when it is considered, that the changes in the medium in which we live, must have an important influence in the condition of the body itself, and on the animals and vegetables by which it is supported. Yet, notwithstanding the multitude of observations which have been made by all classes of men, and the deductions, in the form of proverbs, that have been derived from them, it is certain, that the immediate, as well as the more remote causes of meteorological phenomena, are still, in a great measure, unexplained. This truth may be made to appear, by glancing at what remains to be known of one of the most common processes connected with meteorology, the rise of water into the atmosphere and its return to the earth in the various forms of dew, rain, hail, and snow. It is not yet decided how this liquid is taken into the air; whether it be raised in vapour by the operation of heat alone, and in that state, rise between the particles of the air, without being subjected to their pressure, or by subsequent combination with them; or whether it be by a sort of attraction or affinity, between the air and water, analogous to saline solution, the atmosphere being regarded as a solvent, and dissolving a quantity of liquid, proportional to its temperature, motion, and dryness. We are ignorant in what form it exists after it gets into the atmosphere; whether it remains water or vapour, or is decomposed and resolved into its constituent principles; we are not well instructed respecting the causes of its precipitation; of the constitution of falling mist or vapour; of the formation of rain, hail, and snow; of the production and disappearance of clouds; or the quantity of vapour actually contained in a given portion of air at a determinate temperature. Of other atmospherical phenomena our knowledge is not more certain, and the nature and causes of meteors, the sources of aërolites, the epidemical constitution of the air at different times, and the influence of electricity in preventing, promoting or modifying these events, are still involved in profound obscurity. In making these remarks, it is not intended to deny that the researches of De Luc, Saussure, Hutton, Berthollet, Van Mons, Rumford, Leslie, Dalton, and of many others, have been of essential service. The observations of these philosophers have unfolded a multitude of facts which must be useful to succeeding meteorologists, and may serve as the ground work, for a system of meteorological knowledge. At the same time, it must be conceded that this department of science is encumbered with many loose opinions, and some fanciful and inadequate hypothesis, which rather retard than assist the inquirer in his investigations. A work professedly devoted to the subjects of meteorology, is comparatively a rare sight. We shall, therefore, endeavour rather to analyze than to review or criticise it, presuming, that after the reader has been informed of the nature of its contents, he will be able to judge without our assistance, whether any thing has been added to his previous stock of knowledge on atmospheric phenomena.

The contents of this book are divided into ten chapters. The first is devoted to the illustration of Mr. Howard's nomenclature and theory of the clouds; a theory which was published some years since in the London philosophical journals, but which has not, until lately, received the attention its ingenuity and plausibility have merited. "Clouds," says Mr. Forster, "are distinguished by seven different modifications, the peculiarities of which seem to be caused by the agency of electricity; for example, three primary modifications, the Cirrus, the Cumulus, and the Stratus; two which may be considered as intermediate in their nature, the Cirrocumulus and the Cirrostratus; one which appears to be a compound, the Cumulostratus; and lastly, the Cumulocirrostratus or Nimbus, a state, which immediately precedes the resolution of clouds into rain."

1. Cirrus. Def. Nubes cirrata tenuissima quæ undique crescat.

This modification is frequently seen high in the air. It is in the form of whitish fibres, increasing in number and size; these fibres or pencils, being arranged, either parallel or diverging, sometimes transverse, interlaced, piliform or comoid. Cirri sometimes change their forms rapidly; at others, they are seen for hours, and even days, without much alteration. Of the uses and nature of this species of cloud, Mr. Forster remarks:

"The observations of Mr. Howard, as well as those which I have made, have induced me to believe, that, under whatever form the cirrus may appear, it must always be regarded as a conductor of the electric fluid. Its very texture seems indicative of its particular office. The long parallel and elevated lines, are probably equalizing the electricity of masses of air, very remote from each other; the detached comoid cirri equalizing their own electricity with that of the surrounding air, while oblique or depending tufts, appear to be conducting from an upper to a lower stratum. The cirrus too, is sometimes interposed, and conducting between two other clouds at some distance from each other,"—"and it is probable, that a cirrus ceasing to conduct, ceases to be a cirrus, and that it either evaporates or passes into some of the other modifications."

2. Cumulus. Def. Nubes Cumulata densa, sursum crescens.

This species of cloud appears at first, like a small irregular spot, and as it moves along in the lower atmosphere, increases in size, becomes more dense, preserves a flat horizontal base, and assumes more or less of a conical figure. The forms of cumuli are various; they are often imitative, and present to the eye of the spectator aerial towers, rocks, and mountains of great magnitude and grandeur. "Before rain, they increase very rapidly, and descend lower in the atmosphere, and become fleecy and irregular in their appearance, with their surfaces full of protuberances." In fair weather, "they form soon after sunrise, arrive at their maximum in the middle of the day, and become very convenient skreens to intercept the rays of the sun; and they subside in the evening."

In accounting for the formation of the cumulus, Mr. Forster supposes, that, by the action of solar heat on the surface of the earth, water is evaporated, the vapour ascends, it presses on that portion of vapour which has escaped precipitation, and causes it to rise higher, by which it changes its climate and is condensed into visible vapour. As the sun declines, the cause ceasing to act, the cumuli disappear.

3. Stratus. Def. Nubes strata aquæ mòdo expansa, deorsum crescens.

The stratus is the lowest of clouds; its under surface usually rests on the earth, or on the water. "It may properly be called the cloud of night." It comprehends fogs and mists, which ascend in the summer season, in sheets from vallies, lakes and fields, and in the autumn and winter continue throughout the day as dense fogs. The formation of the stratus is supposed to be owing to the diminished temperature of evening, the air depositing a portion of its water, and the cumuli of the day sinking into the stratus of the night. "The stratus is found to be electrified positively, and to be highly changed."

4. Cirro cumulus. Def. Nubeculæ densiores subrotundæ et quasi in agmine adpositæ.

When the cirrus ceases to conduct electricity, it either disappears, or it loses its cirriform and fibrous structure, descends lower in the atmosphere, and assumes the form of well defined and roundish little clouds, lying in a close horizontal arrangement. It then constitutes the cirrocumulus. It is supposed to have acquired a strong positive charge. "It is frequent in summer, and often forms very beautiful skies; at all times in the year, it may be observed in the intervals of showers, of which its prevalence is a pretty certain prognostic."

5. Cirrostratus. Def. Nubes extenuata sub-concava vel undulata. Nubeculæ hujus modi adpositæ.

When the cirrus descends lower in the atmosphere, when its fibres become denser, and in general, more regularly horizontal, and it usually appears subsiding or altering its form, it is then changing or changed into the cirrostratus. "The figures of the cirrostratus, like those of the stratus, are very various; sometimes it consists in dense longitudinal streaks; at others, it looks like shoals of fish; sometimes, the whole sky is so mottled with it as to give the idea of the back of the mackerel; this has been called the mackerel-sky; frequently it appears like grains of polished wood, or is composed of fine fibres, disposed after the manner of the fibres of muscles, which often intersect each other."

6. Cumulo stratus. Def. Nubes densa irregularis, basin planam, undique supercrescens.

"The Cumulus, losing its hemispherical figure, increases irregularly upward, grows more dense and overhangs its base, in uneven ragged folds; a pre-existing cirrus, cirro cumulus, or cirro stratus, or one perhaps, immediately formed for the occasion, alights on its summit and inosculates. Cumulo-strati frequently remain in this state a long time, and constitute very picturesque skies." From observations on the process of "nimbification," Mr. Forster is of opinion, that it consists in the neutralization of the electricities of two or more clouds. Sometimes, before thunder-storms this cloud has a reddish appearance, which has been thought by some, to be owing to a high electrical change.

7. Nimbus. Def. Nubes vel nubium congeries, pluvium effundens.

This is the rain-cloud. The process of its formation in showers, is as follows:—The cumulus, sailing along in the lower atmosphere, appears retarded in its progress, increases upward, inosculates with a cirrus or cirro stratus above; then the whole changes into cumulo stratus, and spreads horizontally, forming a dense sheet; a sort of crown of cirrose fibres extends upward from the superior part, while loose flocky cumuli, entering from below, seem to nourish the growing nimbus, which increasing in density, descends in rain. After the shower, the cloud breaks, the cirrus, cirro cumulus and cirro stratus are seen high in the air, and the remaining nimbus, in the state of loose flocky, and dark coloured cumulus, flies along below in the form of scud.

The remainder of the chapter is occupied by remarks on the hypotheses, which are opposed to the theory of Mr. Howard, and which we shall not stop to examine.

The next chapter contains remarks on the varieties of the clouds above mentioned; their structure, elevation, colours, and anomalous appearances; on thunderstorms; on the phenomena of hail, rain, and snow; on the Halo Parhelia and rainbow; and on evaporation. These subjects are very cursorily treated of, and do not require much notice. The most interesting part is that, which relates to the influence of opposite states of electricity in bringing together the particles of vapour, constituting different clouds, and the gradual passage of these modifications from the cirrus to the nimbus.

Mr. Forster describes three kinds of meteors. The first is that which is commonly called *shooting star*. It is usually seen in clear frosty weather, and passes through the air without leaving a train. The second is larger and more brilliant, appearing in summer evenings, particularly when cirrocumuli, cirrostrati, and thunderstorms abound. The third is generally small, of a beautiful bluish white colour, and leaves long white trains behind, visible for some seconds in the track of the meteor. It is generally seen in the intervals of showery weather, and from remote antiquity has been regarded as a prognostic of high wind. Of the origin of these meteors and of the aërolites, or meteoric stones, which are mentioned in the next section, we have no knowledge.

In the two succeeding chapters, the author has collected, from the writings of the ancients, from verbal information, and from personal observation, a great number of prognostics of the weather, the season, &c. derived from the instincts and motions of animals and plants, and the appearances of the sky. He has terminated this subject, by inserting a multitude of proverbial prognostics from the works of Bacon, Ray, White, &c. very candidly adding, His omnibus ex ingenio suo quisque demat vel addat fidem.

The subject next treated of is one, which is much more interesting to the Faculty, viz. the influence of certain states of the air in producing epidemical diseases. Ever since the time of Sydenham the prevalence of such disorders has been thought to depend on some peculiar state of the atmosphere. In what this constitution of the air consists, we are, as yet, ignorant. It has often been ascribed to changes of temperature, and alternations of dryness and moisture; some have supposed it to be owing to the presence of some foreign principle in gas, diffused in the atmosphere. But all this is hypothetical. It is obvious that, at present, we have not sufficient data to allow of the adoption of an exclusive opinion on the subject; for the air, which is thus capable of predisposing the human frame to diseases of a peculiar nature, and which varies in different years, contains no foreign matter which can be detected by the most rigid chemical investigation; it is possible that it may; and that it has hitherto escaped discovery in consequence of the extreme subtlety of the matter itself, and the imperfection of the modes and the comparative rudeness of the instruments, by which the analysis of the air is effected. Still as its presence has not been demonstrated, it can hardly be regarded in the light of facts. Mr. Forster, in theorizing on this epidemical constitution of the air, supposes it possible that it may depend on some inexplicable peculiarity in its electrical state. He observes that, during the prevalence of unhealthy weather, he has remarked circumstances, which appeared to denote an irregular distribution of the atmospheric electricity; such as the manner of distribution, and continual and multiform changes of the cirrus cloud; the rapid formation and subsidence of the cirrocumulus and cirrostratus in different places, and the irregular appearance of the other modifications; the intermittent action of De Luc's aërial electroscope; strong and varying winds; and abundance of meteors by night. Its modes of action, however, he thinks are not easily ascertained. These atmospheric peculiarities may deprive persons, already weak, of a portion of their electricity, and thus the energies of the brain and nervous system may be diminished; or the atmospheric electricity being unequally distributed in the air, or propagated downward at intervals, may occasion an irregular distribution of it in our bodies, and produce an irregularity of function. He hints at the probability, that the moving power of the system is electricity, an opinion which has been eloquently expressed by Mr. Brydone, and more lately by Mr. Abernethy. Mr. Forster thinks, that even the diseases peculiar to the seasons and not contagious, may be referred to the action of this principle, in conjunction with peculiar states of the air, which may operate on the body as specific stimuli. Epidemics also occasionally appear in animals and plants; and the subject is closed by the author by the remark, "How far electricity may be concerned in all this, it is difficult at present to say; but the discoveries which philosophers are daily making, relative to the extensive operation of this fluid, (for such I must call it, till a better name be found) seem to encourage a suspicion that its agency is concerned in producing every change in the universe." With this remark, which is consigned to the examination of the reader, we leave the subject, and pass to the other portions of the work.

1. *Electrical state of the Clouds.* After the union of clouds oppositely electrified, the nimbus which is formed may acquire a strong positive charge, and exist as an highly electrified conductor; and it is probable, that the difference between common showers and thunderstorms, consists in the intensity of the charge, which the numbers acquires after its formation. But Mr. Forster thinks, that the theory is insufficient to account for the formation of the other modifications of clouds; and that experiments and observations are still wanting to determine the real influence of electricity, in the production of these phenomena.

2. *Thunder and lightning.* Two kinds of thunder have been noticed by meteorologists, the one a rattling, exploding sound, following a vivid flash of lightning; the other rolling and heavy; the former is mischievous, the latter innocent. The first is ascribed to the passage of electrical matter from

the clouds to the earth, or from the earth to the clouds; the second, by many meteorologists, to the combustion of oxygen and hydrogen gases in the atmosphere. But what proof, we may ask, is there, that hydrogen gas, in an uncombined form, exists in a gaseous state in the air? None. The opinion, although it has been maintained since the discovery of oxygen gas, is entirely gratuitous, hypothetical, and improbable. Should azote or nitrogen be proved to be a compound of oxygen and hydrogen, or, in other words, an oxide of hydrogen, as it is conjectured to be, this idea might appear less extravagant. Is it not most probable, that the rolling protracted thunder may be owing to the passage of a stream of electricity from cloud to cloud? Mr. Forster thinks, that most of the flashes of lightning never reach the ground.

3. *Silent lightning.* This electrical phenomenon, which is known among us by the name of *heat lightning*, and which is observed in warm and calm evenings in summer, is ascribed by the author either to clouds too distant to be observed, or to electrical communication between the damp earth and the hazy air above; the vapour then existing in the atmosphere, and the moisture on the surface of the ground, conducting it in a silent and gradual manner from one to the other.

4. *Aurora Borealis.* It has been the general opinion, that this phenomenon is connected with electricity, and "the reason why these Auroræ only happen at the poles has been said to be because, in high latitudes alone, there can be a continual coating of ice and snow to prevent the electric communication taking place between the earth and the atmosphere above. The aurora has, also, been explained on the supposition that the earth may have electric polarity; and the correspondence between the centre of the aurora and the magnetic poles, which has been asserted, is a circumstance very worthy of future consideration, as it tends to establish more certainly the connexion between electricity and magnetism." The most recent opinion on this subject is, that the beams of the aurora are magnets, or magnetic cylinders; and the idea is supported by the fact, that their position varies with the declination of the needle. The altitude of these beams differs, exceedingly, at different times. In 1751, Femerus of Upsal, calculated their height at 1006 miles, while those which appeared at London, in February, 1784, were estimated, by Mr. Cavendish, to amount in height only to 62 miles. The mean height, as calculated from observations on 31 different auroræ, is 476. 709 miles.

5. *Electricity of the air.* Mr. Forster has not added much to this subject.

6. *De Luc's aërial electroscope,* more commonly known by the name of the electric column, is a very ingenious and philosophical instrument, the construction of which was undoubtedly suggested by the discoveries in voltaic electricity. As its structure and modes of operation are curious and interesting; as it is affected by variations in the electrical state of the atmosphere, may be made subservient to the pursuits of the meteorologist, and formed without much difficulty, we shall describe it in the words of Mr. Forster, referring those who may wish a more accurate account to De Luc's paper in the Philosophical Journals of London.

"It is composed of a great number of small, circular, and very thin plates, about the diameter of a sixpenny piece, of zinc and paper, alternately arranged, and forming a column, the two ends of which are made to approximate, and at each of them is attached a small bell; a metallic clapper is then hung between them, and the whole apparatus is then insulated by being fixed on glass stands. One end of the column is observed to become electrified *plus,* as it is termed, and the other *minus;* consequently one of the bells becomes electrified *plus* or positive, and the other *minus* or negative; and the metallic clapper moving rapidly from one to the other, to neutralize the electricities, a pulsation is produced, and the bells ring. Neither the heat nor cold, dryness nor moisture of the atmosphere appears to have any considerable influence on the action of this instrument, but it is considerably altered by peculiarities in the electric state of the atmosphere." When the circumstances already mentioned indicate a disturbed state of atmospheric electricity, the action of the electroscope is irregular; if rain succeed, it is silent. "Sometimes they ring weak and regular, sometimes weak and irregular; sometimes strong and regular, at others strong, but irregular; the intervals of quiescence are sometimes longer than at others." "When the weather is settled, when only diurnal cumuli prevail with westerly winds, then the action of De Luc's column is the most regular, and this is found to be the most wholesome weather." These columns have been kept in action in this way two or three years.

The remainder of the book consists of further investigations on the peculiarities of weather; miscellaneous observations on temperature and pressure of the air; and superstitious notions, which appear to have their origin in an observance of certain meteorological phenomena; with an appendix, including observations on diet, as connected with the influence of atmospheric diseases; and quotations from authors who have written on meteorology. As this analysis, however, has been already extended to some length, and embraces the most important and novel part of the work, it is thought unadvisable to prolong it.

The fault of Mr. Forster is in writing on too many subjects. Had the work been confined to the nomenclature of the clouds, with the theory of their formation and dissolution, and to the phenomena immediately connected with them, it might have been rendered not only more interesting, but also more useful; but divided as it now is, into numerous sections, and almost every section presenting a new subject, the space he has allotted for each is too limited; and many of the curious and important phenomena of meteorology are passed over, in a very cursory and unsatisfactory manner. It is to be hoped, that the author considers it merely as the skeleton of a much larger and more perfect work; and as he has here shown himself, as we knew him before, to be a zealous supporter of the doctrines of Messrs. Gall and Spurzheim, we trust he has discovered that the cranium of man exhibits an organ of meteorology, which may be developed in proportion as the genius it gives, is cultivated. We do not, indeed, find such an organ in the catalogue of Dr. Spurzheim, but it is easy to make one, and then we need not fear that the outlines of Mr. Forster, will remain in the meagre state in which they are now exhibited.

Before closing this article, we beg leave to offer one remark. The perusal of this work has afforded another proof, that electricity is rapidly rising in importance, as a general agent in the operations of nature. Its influence in the production of many of her grand and sublime phenomena is admitted, and we recognise its agency in the thunderstorm, the water-spout, the whirlwind, and the earthquake. It is supposed to be equally concerned in exciting the motions of matter, on which depend the changes in the composition of inorganic bodies. The discoveries and researches of Sir Humphrey Davy have rendered it probable, that affinity is connected with the electrical energies of bodies; and that those which exhibit the most opposite properties, and have the greatest tendency to combine, are such as discover opposite electrical attractions. This hypothesis, much amplified, has been made by Berzelius, the foundation of a new theory of chemical science. Nor is this all. There are some physiologists of great name, who have more than hinted at the probability that electricity is the moving principle of the living system, and endeavoured to identify the nervous and electric fluids. Whatever may be said about the influence of Lord Bacon's inductive process, in restraining the mind from hasty and imperfect conclusions, it must be acknowledged that, with regard to physics, this is the age of novelty, hypothesis, and theory. Whether electricity will ever be made the key-stone of one great, uniform, and beautiful system, time alone can unfold to us. At present the properties, the phenomena it produces, and the modes of action of this principle, constitute very interesting subjects of research; and he who labours in this field, can scarcely fail of reaping his reward.

As the reader is now, probably, fatigued with his aërial excursion, we shall replace him on *terra firma* and retreat.

INTELLIGENCE.

Domestic.

HARVARD UNIVERSITY.

STATUTES IN RESPECT TO MEDICAL INSTRUCTION AND DEGREES.

I.

THE FACULTY OF MEDICINE of this University shall consist of the President, and of the Professors and Lecturers authorized to give instruction to the medical students. This faculty shall always have a Dean elected by themselves, for such periods, as they may think proper, and may also adopt rules for their own government, provided that the same do not, in any respect, contravene the laws of the University.

II.

Students of medicine, designing to attend the medical lectures, or any of them, shall be matriculated in this University, by entering their names with the Dean of the faculty of medicine, to be enrolled by him; and by signing an obligation to submit to the laws of the University, and to the direction of the faculty of medicine.

III.

There shall be four meetings holden in this University annually, by the faculty of medicine, for the purpose of examining candidates for the degree of doctor of medicine. Two of these meetings shall be for private examinations, and shall be holden in the Massachusetts Medical College in Boston; and two of them shall be for public examinations, and shall be holden in some one of the halls of the University in Cambridge. Three members of the faculty at least shall be present at every examination. The first meeting for private examinations in every year shall be holden on the day, next succeeding that on which the winter courses of medical lectures shall terminate, at ten o'clock A. M. The second meeting for private examinations shall be holden on the Monday next but one preceding the day of the annual commencement in the University, at ten o'clock A. M. In extraordinary cases the faculty may hold meetings for private examinations at other periods. The meetings for public examinations shall be holden on such days as the President may appoint, provided that the same shall take place within one week after the termination of the stated annual meetings for private examinations respectively. All the meetings described in this statute may be continued by adjournment. The meetings for the public examinations shall be open to the Governors and instructors of the University, to the fellows of the Massachusetts Medical Society, and to such other respectable persons as may choose to attend them.

IV.

Candidates for the degree of Doctor of medicine must comply with the following conditions, before being admitted to private examinations, viz:

1. They shall have attended two courses of the lectures delivered at the Massachusetts Medical College on each of the following subjects, viz:

Anatomy and Surgery—Chemistry—and the Theory and Practice of Physic.

2. They shall have employed three years in their professional studies, under the direction of a regular practitioner of medicine.

3. Those, who have not received a University education, shall satisfy the faculty of medicine in respect to their knowledge of the Latin language and experimental philosophy.

4. Every candidate, intending to offer himself for private examination shall, three weeks previously, give notice of his intention to the Dean of the faculty, and shall at the same time deliver, or transmit to the Dean a dissertation, written by himself, on some subject connected with medicine. Every dissertation shall be submitted, by the Dean, to the examination of the faculty in the mode which they shall point out.

V.

At the meetings for private examinations, the faculty shall examine all those candidates, who shall present themselves, after having complied with the conditions enumerated in the fourth of these statutes, upon the following branches of medical science viz. ANATOMY, PHYSIOLOGY, CHEMISTRY, MATERIA MEDICA, PHARMACY, MIDWIFERY, SURGERY, *and* THE

THEORY AND PRACTICE OF MEDICINE. At these meetings every candidate shall be examined separately, and the decision of the faculty in respect to each, shall be made and declared to him immediately after the examination has closed. The decision in respect to each candidate shall be determined by the votes of the major part of the members of the faculty, present at the examination of the same; and this decision, if favourable to the candidate, shall be recorded by the Dean. In the decisions to be made at these meetings, regard shall be had to the dissertations, as well as to the examinations.

VI.

Those candidates, who have been approved according to the fifth of these statutes, may present themselves at the public examination, next ensuing after such approbation. Each candidate, so presenting himself, shall then read and defend, or be examined upon the dissertation, which he shall have previously submitted to the faculty. At the close of each public examination, the faculty shall decide, in respect to each candidate, whether he shall be recommended as worthy of the degree for which he has applied. The decision of the faculty in respect to all those candidates, whom they do so recommend, shall be recorded by the Dean and shall by him be certified to the President, to be laid before the SENATUS ACADEMICUS. The candidates will learn the decisions in respect to them by application to the President.

VII.

Those candidates, who have received from the *Senatus Academicus* the final approbation, will be directed by the President to appear at Cambridge, at such time as he may appoint, and he will then admit each of them, with the accustomed solemnities, to the degree of doctor of medicine.

NOTE.

THE lectures for medical students on the various branches mentioned in the statutes, are delivered at the Massachusetts Medical College in Boston, and commence annually on the third Wednesday in November. They continue three months. During the lectures, the students may find in the town various opportunities for practical instruction.

The Hollis Professor of natural philosophy will admit medical students to attend the lectures on natural and experimental philosophy. The lectures are delivered in the Philosophy Hall at Cambridge, four days in the week, between the middle of March and the middle of July annually. In order to their admission such students must produce, to the Professor above mentioned, a certificate of their matriculation from the Dean of the faculty of medicine, and another certificate from the Steward of the University that they have paid him seven dollars for the Treasurer. Other persons may be admitted to the same lectures, with the approbation of the President, and producing a certificate from the Steward that they have paid ten dollars. Application to be made in writing to the College Registrar.

The fee for the degree of Doctor of medicine is to be paid to the College Treasurer. The fee is twenty dollars for a person who has not taken a degree of Bachelor of arts at any College or University; fifteen dollars for one who has taken the degree of Bachelor; and ten dollars for one who has taken the degree of Master of Arts.

It is to be desired that all candidates for the degree of doctor of medicine should comply with the foregoing statutes; but in cases where they operate as *expost facto* laws, and thereby occasion any considerable inconvenience to candidates, exemptions will be made by the faculty of medicine.

FACULTY OF MEDICINE.

REV. JOHN THORNTON KIRKLAND, D.D. and L.L.D.
President of the University.

JAMES JACKSON, M. D. Professor of the Theory and Practice of Physic.
JOHN C. WARREN, M. D. Professor of Anatomy and Surgery.
JOHN GORHAM, M. D. Professor of Chemistry.
WALTER CHANNING, M. D. Lecturer on Midwifery.
JACOB BIGELOW, M. D. Lecturer on Materia Medica, and Rumford Professor in the University.

The REV. Dr. WARE is Registrar of the University.
Dr. WARREN is Dean of the Faculty of Medicine.
WM. GAMAGE, jun. M. D. is Librarian of the Medical College, and
ABNER HOWE, A. B. Sub-librarian.

Foreign.

Extract from an Essay on the Bad Effects of the Incautious Use of Magnesia. By Everard Brande, Esq.

A lady was recommended to take magnesia, in consequence of some very severe nephritic attacks, accompanied with the passage of gravel. She was desired to take a tea-spoonful every night; and Henry's calcined magnesia was preferred, as that always operated upon the bowels and "carried itself off," which other magnesia did not, but, on the contrary, felt heavy and uneasy in the stomach. The dose was gradually increased to two tea-spoonfuls, in order to produce effect upon the bowels, which this quantity never failed to do; the symptoms for which it was ordered were soon removed, but the plan was persevered in two years and a half, with little intermission or irregularity; so that as the average weight of a tea-spoonful is at least forty grains, and the average dose was a tea-spoonful and a half, it may be presumed that she took during the above period between nine and ten pounds troy.

In the course of the last autumn she suffered severely by a miscarriage, and shortly afterward by an attack of biliary calculi; subsequent to which she became sensible of a tenderness in the left side just above the groin, connected with a deep seated tumour, obscurely to be felt upon pressure, and subject to attacks of constipation, with painful spasmodic action of the bowels, tenesmus, and a highly irritable state of stomach; these attacks recurred every two or three weeks, varying in violence, but requiring the use of active remedies; during one of them, about the middle of last March, a large, quantity of sand was voided by the rectum, attended with a peculiar acute and distressing pain in the seat of the tumour above mentioned. This was lost. The following day, however, the same kind of evacuation, happened again, and to the same extent, which being saved and measured, was found to amount to two pints. Another attack took place upon the 5th of April, when several irregular lumps of a soft light brown substance were voided, having the appearance of a large mass broken down, and when dry extremely friable: a part of each of these two last were subjected to a careful analysis, and found to consist entirely of sub-carbonate of magnesia concreted by the mucus of the bowels, in the proportion of about 40 per cent.

The use of magnesia was now given up, and that of an active purgative medicine enjoined, with some other necessary directions, and there is every appearance of returning health, although some slight attacks have recurred, and small portions of the same concretion still occasionally come away.

An instance, in many respects resembling this, has lately occurred in the practice of some gentlemen of eminence in this town, in which not only large quantities of a concretion of a similar description were voided, but upon examination after death, which took place perhaps six months after any magnesia had been taken, a collection, supposed to be from four to six pounds, was found embedded in the head of the colon, which was, of course, much distended. Some notes which were made of this case are, I fear, not to be found.—*Journal of Science and the Arts*, vol. i. p. 299.

PRACTICE OF MEDICINE.

I.—Syphilis is occasionally observed in infants, and it becomes a question of some importance to ascertain, whether, in the event of the nurse not being the mother of the child, the disease has been communicated by the child or the nurse. The following paper, by *M. Cullerier*, which was read to the Société de Médecine de Paris, in January last, is drawn up expressly with the view of laying down rules, by which the fact may be determined. In translating it we have left out such of the cases as appeared the least important. The paper is intitled "*Observations sur la Contagion Syphilitique, dans les Rapports des Nourrices avec les Nourrissons.*"

"Symptoms of syphilis are often observed in infants many days, weeks, or months, and sometimes even years, after birth; and it frequently happens that the disease is communicated by an infant to the nurse by whom it is suckled. In the hospital *Des Veneriens* nurses are occasionally found who have been affected in this manner: but it is, also, not uncommon to see nurses who, either by their own misconduct, or that of their husbands, have communicated to their nursling the virus with which they are tainted. I am occasionally consulted, both by nurses and the parents of nursed children, regarding this disease, and to decide whether the infant or the nurse have communicated it. The following observations are intended for the government of those who may be required to decide in cases of this description, particularly when they are brought into a court of justice. I will commence with a case which is still pending in one of the civil tribunals of Paris.

"*Case first.*—About three months ago the President of the Court *des Vacations* brought before me, at the request of the counsel of the parties, a nurse with her nursling, to be examined as to the state of their health. I made the following report.

"——— certifies having found upon M** C**, the wife of M**, a nurse—1°. some blotches upon the breast, of a bright brown colour;—2°. others nearly of the same hue, some of which were covered with thin scales, on the right side;—3°. three or four spots resembling the preceding, with a small tubercle, at the posterior part of the neck;—4°. seven or eight small, widely scattered blotches on different parts of the body: and he is of opinion that these blotches, the scaly spots, and the tubercle, are the probable symptoms of the existence of a secondary syphilis. I added, that the nurse had assured me that she had had ulcers in the mouth and on the breasts; but I could perceive no traces of them.

"——— certifies that the infant of Sir and Lady L**, the child nursed by M**, aged about one year, has—1°. a round superficial ulcer on the inside of the upper lip, near the fræenum;—2°. another foul ulcer, nearly circular, and of two or three lines in diameter, situated on the perineum; and that these ulcers present the characteristics of those of a primary venereal affection.

"——— certifies finally, that not having seen the disease in its commencement, either in the nurse, her husband, the infant, or its parents, it is impossible to decide positively whether the infant communicated or received the infection.

"The tears and asseverations of the nurse interested me in her favour; but the symptoms were such as to raise doubts in my mind, which I was anxious to remove; and therefore I examined the parents, on whom I could discover no signs either of a present affection, nor cicatrices of past disease.

"At the first visit, the nurse informed me that one of her daughters, a girl fourteen or fifteen years of age, had a sore throat, which she supposed she had caught from kissing the infant. I regretted that I could not examine her, as she was left at home, a distance of seven or eight leagues from Paris. About three weeks afterwards, however, the nurse and her daughter were brought to me by a legal warrant. The former appeared in a state of health; but on examining the mouth of the daughter, I found the amygdalæ ulcerated; and on the left labium pudendæ, a large tuberculous pustule. If I had made no further inquiries, there was reason for concluding that the disease was communicated rather by this girl than by the infant;

and this conclusion was confirmed by an examination of the vulva. I found some small ulcers on the interior of the nymphæ; the hymen was lacerated; and the entrance of the vagina was of a size sufficient to suggest the idea that the girl had had frequent sexual connexion, which she afterwards confessed was the fact. From this discovery it was evident that the ulcerated mouth of the child had been produced by the reiterated kisses of the daughter of the nurse; and it was probable that the ulcer on the perineum had proceeded from its having been moistened by her saliva, a custom which is not uncommon among nurses. I did not acquaint either the father or his counsel with my opinion; but requested that the daughter should be examined by my colleague before being admitted into the hospital *Des Veneriens*, where she wished to be placed. I saw no more of her.

"Where lay the culpability? On one hand, an infant a year old has two simple, superficial, primary ulcers; but the parents are free from any symptom of syphilis, or any mark of previous disease: on the other, the nurse has suspicious symptoms, or rather symptoms of a secondary affection. Her daughter has ulcers on the amygdalæ, a large pustule on one of the labia, and ulcerations on the nymphæ; and although still young, bears evident marks of having had frequent sexual intercourse. Notwithstanding these proofs of culpability in the nurse's family, she pursued the parents of the child, for damages. The opinion of the public, and that of the judge, was in favour of the guilty, merely because, said they, country folks are not corrupt enough to complain without a cause.

"*Case second*, is another instance of a child being affected, and the disease traced to the nurse.

"*Case third.*—M. M. Marchais and Hallé had attended Madam M——, and her infant: the one as an accoucheur, the other as a physician. This child, which was suckled at home by a Parisian nurse, had, a few months after birth, a gathering, and some slight ulcers in the mouth. Pus was discharged from the nose; the respiration became difficult and laborious, and the strength gradually decreased. The infant was in this state when I was requested to see it. Neither the physician nor the accoucheur had perceived any symptom of a venereal affection upon the parents; but the father acknowledged that he had had that disease before marriage; assuring us, however, that he had been completely cured. M. M. Hallé and Marchais had not perceived any thing on the infant for some months after birth, except a faint brown spot upon the scrotum, which almost entirely disappeared without any topical application.

" The nurse had some superficial ulcerations on the breasts and some mealy and scaly eruptions on the chest and arms. The genitals were not examined, as we were unwilling to awaken any suspicion; and, after the avowal of the father, and the brown spot on the scrotum, it was probable that the disease was hereditary. I was not, however, of this opinion, as I found the blotch on the scrotum insignificant; and, from the cutaneous affection of the nurse, I was persuaded that she had affected the child. It was nevertheless, agreed, that my suspicions should not be hinted to the nurse, and that her disease should be called tetters (Dartres.) The plan of treatment was agreed upon; but the infant, being too much exhausted, died, without having been able to take any medicine.

" The parents, being fully persuaded that the nurse had been the victim of a disease received from them, engaged her to remain with them after the death of the infant, to be cured of her tetters. She consented to this proposal, and I was desired to attend her. Before beginning, I requested to be made acquainted with the state of the whole body; but she objected on account of the menses being upon her. Some days afterwards I renewed my request, when she again put off the examination for two days; and next day, having declared that she would go home, she thanked Mr. M. for all he had done for her, and proposed to do, but refused to wait upon me. She has never since re-entered her master's house. Would she have conducted herself in this manner, if she had not been culpable? It has since been ascertained that her husband, the driver of a fiacre, is an abandoned character.

"The *fourth case* resembles the third in some particulars. The child died; and, although it was known that the husband of the nurse, and the nurse herself, had been lately treated for syphilis, until the symptoms had disappeared, yet the parents of the infant kept her for three months, until she was fairly cured; whilst they themselves remained in perfect health. M. Cullerier remarks, 'that every day's experience proves that the symptoms disappear spontaneously, although the virus still remains in the system.'

"*Case fifth.*—About two years ago, a nurse brought to me an infant, six or eight weeks old, upon the labia and anus of whom there were some incipient pustules. I informed her that the child had symptoms of a contagious disorder, and desired her to acquaint the parents with my suspicions. The genitals of the nurse were free from any diseased appearances, nor were there any upon the nipples. Eight days afterwards the nurse returned, and brought with her, her nursling, the father and mother of the infant, and a physician. The symptoms on the child had become less unequivocal, and some ulcers had appeared upon the breasts of the nurse. The parents wished to throw the blame upon the nurse, alleging that they were in good health, whilst she had ulcers upon her breasts: but I stated, that when she waited upon me, eight days before, the nipples were in a healthy state, although the nursling was evidently under the influence of the disease. They agreed to take the infant from her, and give her an indemnification, which she accepted; but the answer of the father to the questions which were put to him, explained the origin of the disease.

"I could relate many other cases of this kind; but they would be superfluous, and the relation would merely be a repetition of nearly the same circumstances.

"The conclusions which the facts authorize, appear to be, 1. That if the breasts of the nurse and the mouth of the child only are at the same time diseased, we ought not to give an opinion as to the origin of the disease until we have other evidences. 2. That if the breasts of the nurse only be diseased, whilst other marks of disease, besides those in the mouth, are apparent on the infant, it is very probable the complaint has been communicated by the child. 3. That if the infant has no disease except of the mouth, and other marks of it appear on other parts of the nurse besides the breast, it is very probable that the infant has imbibed the disease with the breast-milk. 4. That if the nurse have secondary symptoms, and the child primary only, the probability is, that the complaint has been communicated from the nurse, or some other person by whom the infant may have been kept. 5. That if the child have secondary symptoms, the disease is probably hereditary. 6. That in doubtful cases, nothing should be neglected which may lead to the discovery of the truth: by examining all the concomitant circumstances, the veil may be generally removed; although it is sometimes necessary to temporize, and to do nothing precipitately."

SCALE OF CHEMICAL EQUIVALENTS.

Dr. Wollaston has done an important service to practical chemists, by the invention of this scale, as it indicates with great precision, the composition of a multitude of salts, and presents at one view a mass of information, which before could be collected only by consulting some large work on chemistry. The following is a description of the sliding scale, by the editor of the Annals of Philosophy.

"It was first observed by Richter, that when two neutral salts are made to decompose each other, the neutrality of neither is disturbed. Thus, if you dissolve one hundred grains of sulphate of potash in water, and pour into the solution, muriate of barytes in sufficient quantity to decompose the whole of the sulphate, two new neutral salts will be formed, namely, sulphate, of barytes, and muriate of potash. If into the muriate of potash thus formed a sufficient quantity of nitrate of silver be dropped to decompose it; two new neutral salts will be formed, namely, nitrate of potash and muriate of silver. Thus the same weight of potash has been successively united with sulphuric, muriatic, and nitric acids. The weight of barytes, that neutralized the muriatic acid, neutralized, likewise, the weight of sulphuric acid, combined with the potash which had neutralized the sulphuric acid. These observations of Richter were still further generalized by Berthollet; but it is to Dalton we owe the full generalization of the facts, and the explanation of them. He supposes that bodies unite atom to atom, and showed how the weights of the atoms of bodies might be ascertained.

Dr. Wollaston has divided the slides of a scale into the logarithmic spaces from ten to three hundred and twenty, by a method which is familiar to all who are acquainted with logarithms, or in a similar manner as the line of numbers is laid down in Gunter's scale. He considers ten as representing the weight of an atom of oxygen. On both sides the scale he has written the substances most familiar to chemists, viz. the acids, bases, water and principal salts, each opposite to the number on the slide, which corresponds with the weight of an atom of it. Suppose, now, we want to know how much oxygen combines with two hundred of mercury; bring two hundred on the slide opposite to mercury in the scale, then over against oxygen on the slide, will be found sixteen, the quantity of oxygen required. Suppose we want to know how much oxide of copper combines with sixty of sulphuric acid; bring sulphuric acid opposite to sixty, and we shall find over against oxide of copper sixty-one, the number required. The sliding rule is fitted to answer an infinite number of similar questions. His scale is referred to carbonate of lime, which he considers as the most exact standard of comparison that can be obtained."

This invention unites simplicity and ingenuity with much convenience. Some of the scales have reached this country, and they will be found useful to every one who attends to the details of chemistry.

POSTSCRIPT.

The following Postscript to the Analysis of the waters of the Congress Spring, was received too late for insertion in its proper place.

SINCE the above analysis was made, I have met with two other analyses of the same water, the one by Dr. Seaman, the other by Dr. Cutbush; I am happy that their results, tend generally to confirm mine. Doctor Seaman found the solid contents of the water to consist of

Carbonate of Soda,	5·2 grains
Muriate of Soda, . .	31·6
Carbonate of Lime, .	38·0
Carbonate of Iron, .	1·7
	76·5

Doctor Cutbush found

Sulphate of Soda,
Muriate of Soda,
Carbonate of Lime,
. . . . Magnesia,
. . . . Iron,

and a portion of Silica, but he was doubtful whether it existed in the water, or was an accidental mixture. I did not find a trace of iron, but the water which I examined had been several weeks from the spring.

J. F. DANA.

Cambridge, January 5, 1817.

The New-England Journal

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Prognosticks in Fevers, adapted more particularly to those of New-England, by JOSEPH COMSTOCK, *one of the Censors of the Rhode-Island Medical Society, Honorary member of the Philadelphia Medical Society, &c.*

[Communicated for the New-England Journal of Medicine, &c.]

IN the new world, such a deviation occurs in diseases from those of the old, that nothing seems more urgently needed, than a treatise to help us in forming an opinion of their terminations. Whether the present will contribute any thing to that effect, must be left to the reader to determine. Suffice it to say, that the remarks, here offered, are the result of considerable experience and attentive observation.

Before proceeding to more minute particulars, I shall submit a few leading and general principles.

First, Habit, which is capable of producing such great changes in every thing relating to man, must come in for a share in our prognosis of diseases; for almost every dangerous symptom, may, by habit alone, lose somewhat of its dangerous tendency, such as hæmoptysis, hæmatemesis, irregular pulse, asphyxia, symptoms of croup, &c.

Second, Hereditary predisposition, and the diseases by which the ancestors of our patients came to their ends, may with great propriety be taken into consideration.

Third, Those sick persons, whose parents were related to each other, as the children of cousins, are not quite so likely to live through violent diseases as others; this observation applies more particularly to childhood.

Fourth, Fever sometimes affects the system unequally, some functions remaining unimpaired, such as the sick person retaining his appetite, not being thirsty, being able to walk about, having a cool skin, a moist tongue, and retaining his attachment to the world and his cares for business. These things are thought favourable by the sick person and his family, but are often the reverse; for the thread of life is cut by the morbid cause acting with concentrated violence upon a particular part, whereas, if the mischief had been more equally divided, and every function had borne a share of the burthen, the healthy tendency of the system might have overcome the disease.

Fifth, Extreme caution is necessary in prognosticating during the reign of mortal epidemicks, for a symptom at other times not esteemed dangerous, will at such periods denote great danger. Instances happened at Hartford, illustrating this remark, when the spotted fever prevailed there. Young women with nothing more than the common symptoms of hysteria, were found sinking into the arms of death with the prevailing disease, thus disguised.*

Sixth, The reverse of this sometimes happens, and symptoms usually fatal will not denote that certain fatality, which we have been accustomed to attach to them: such as entire absence of the pulse, and loss of sight, in spotted fevers; which have not, as we are assured, always proved fatal symptoms.

Seventh, Circumspection is necessary on another account, for during mortal epidemics, unfavourable symptoms lie hidden and ready to invade when least expected, even when many favourable symptoms have flattered us with hopes of recovery. Such diseases carry on an irregular warfare against the human species, and resemble the barbarous nations of the earth, as the Algerines in Africa, and the Indians of the American wilderness, who pillage and destroy after having given assurances of peace and friendship. Remedies, upon such irregular diseases, resemble treaties with these nations; for although they feel their effects and in some measure seem to be under their control, yet when one symptom is alleviated, another as bad or worse seems to lie in ambush, ready to assail, with more or less violence, when least expected.

Having finished these premises, we will now speak of prognosticating from certain signs, as displayed by the effects of fever upon different parts and functions of the human body.

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* See Dr. Strong's Inaugural Dissertation.

I. *Of the countenance and external parts.*

1. A countenance expressive of distress and anguish, sometimes denotes a more alarming state of the patient than can be otherwise discovered. One of the eyes appearing smaller, or more sunk than the other, is almost an uniformly fatal sign. I do not know, that I have ever known but one single exception.

A sharpness of the features, and extreme paleness, are highly dangerous.

Persons, who in sickness resemble their ancestors more than usual, may be suspected of having the soft parts extremely wasted, as this resemblance may have its existence only in the bones.

Sleeping with the eyes partly open is unfavourable, but not always fatal. Hippocrates is said to have considered it a sign of death if the white of the eye was not covered in sleep, unless the patient had diarrhœa, or was accustomed to sleep in this manner in health. Dr. Zimmermann remarks, that he saw Baron Haller ill and asleep in this manner, but that he happily recovered. The eyes being glazed or having a glossy appearance, is usually fatal. I have known this to be the first symptom of death.

Yellowness, and spots on the skin in the fevers not strictly eruptive, are dangerous: the darker the colour, the greater the danger.

3. A continuance of the plumpness of the body and no apparent wasting of the flesh in fevers, is rather a rare but very bad symptom. The whole weight of the disease being diverted from the skin, fat, and muscles, falls with increased force upon the viscera, and the patient is suddenly and unexpectedly destroyed.

4. A reddish, purple, or livid colour of the hands, with coldness, is, as far as I have observed, uniformly fatal. The settling of blood under the finger nails, which is so often looked for by the people, is very fallacious. It is remarkable, that the coldness of death is most apt to begin about the wrists, and is first discoverable at the ends of the radius and ulna, on the sides, corresponding with the thumb and little finger.

5. A bloated, strangulated countenance, thickening of the lips, sallow, yellow, or sub-livid complexion, denote much danger, as does a swelled abdomen, and a leaden colour of the face.

6. A soft, flabby, velvet-like feeling of the skin and muscular parts, denotes such a loss of tone and energy as to lead

us to augur unfavourably. This velvet-like feeling of all the soft parts is in some dangerous cases of ataxic fever, the only sign of alarm. I have noticed it most frequently in children.

7. A depressed, dead, sunken look of the eyes, denotes the body to be extremely wasted, or otherwise an affection of the brain. In persons who are about, however, it denotes hepatic affection, as in the exiled emperor of France and many others.

II. *Of the respiration.*

A rattling of saliva at every breath without any attempt, or without the power to remove it, is one of the most certain signs of speedy fatality; and if accompanied with a drawing down of the under jaw at every breath, the patient must be considered as in the article of death.

A sighing and interrupted breathing, as also snoring respiration, denote such an affection of the brain as is irrecoverable. Very quick breathing, like that of a person after running, bears a deathly aspect.

The number of times which we breathe in a minute, or given space, is reckoned in the proportion of one fourth of the number of pulsations; but this is thought by Baron Haller not to hold good in diseases.

Boerhaave accounts small respiration a dangerous sign. Hippocrates observes, that great breathing is great externally, and small internally; which is exactly correct; for great external breathing, when the breast heaves, and the shoulders and ribs are elevated, is accompanied with very little dilatation of the lungs; whereas a full dilatation of the lungs is accompanied with scarce any motion of the sternum ribs or shoulders.

This laborious respiration may arise, however, from other causes than extreme debility; as from a tense abdomen, or effusion into the cellular substance of the lungs, and even from less dangerous causes; as from costiveness, flatulency and asthma.

It is said by Zimmermann, that Hippocrates, Galen and Alpinus, considered a slow and small respiration, when the patient could hardly be perceived to breathe, as the forerunner of death, and decisively mortal. This, however, must be strictly applied to fevers, or other diseases beside hypochondriasis and hysteria, in which this, as well as the laborious respiration before mentioned, happen with impunity.

III. *Of the motions of the body.*

1. Turning frequently as though there was no easy place in the bed, and rising upwards as though about to get out of it over the pillows, are highly dangerous motions.

2. Tossing about, and drawing the bed-clothes frequently, as though they could not be any how made to lie in the desired form, picking the bed-clothes, or a feather, rag, or any such-like substance, and picking or pulling parts of the body, are all extremely unfavourable.

3. Rolling the eyes, grating the teeth, stretching out the limbs unnaturally, are of bad import, and when other circumstances render a case doubtful, these are sufficient to decide it unfavourably. Smacking the lips, in a certain fever described in the work of Dr. Lind, was esteemed fatal. In the *ataxic* fevers of late years, called spotted, and by other names, and particularly in pneumonia typhoides, a very unusual symptom has been noticed, and which when occuring early, has, as far as I have observed, proved uniformly fatal; this is scratching the head. The first time I noticed it was in a lady, whose habits forbade any suspicion of its arising from an external cause, had not her extreme illness also forbade it. The disease proved fatal to her; and I was led from that and several other cases which occured, to suppose, what I now believe to be the fact, that it arose from effusion, or erysipelatous inflammation of the brain or its meninges.

4. The under jaw being put in motion in breathing, interminable hiccups, and constant tenesmus, are fatal.

5. Flatulence, evinced by borborygmi, belching, and breaking wind, is favourable, and sometimes denotes recovery when almost every thing else points toward the grave.

6. For a sick person to wish to sit up, when entirely unable, for him to wish to be removed into another room, or to be carried home when already at home, are all unfavourable and often fatal symptoms.

7. A tremuor of the under lip, arms, hands and chin, denotes the presence of a very severe disease, and in the fevers of New-England appears to have taken the place of subsultus tendinum, which however occasionally happens, and is of much the same import. A rigidity of one or more of the limbs is generally an attendant on approaching dissolution.

8. According to Galen and Duretus, convulsions succeeding delirium are fatal: but Dr. Zimmermann, who tells us this, remarks, that he has seen it happen and the patient do well;

and this in one instance has been the result of my own experience, but in others it has proved fatal. Convulsions following stupor, I esteem to augur unfavourably.

IV. *Of the Excretions.*

1. Universal or partial hæmorrhages which cannot be checked, unceasing puking or retching, excessive and constant sweats, all usually prove fatal, as does unconscious diarrhæa.

2. Of *diarrhœa*, in general, it may be affirmed, that it is most dangerous in pneumonick affections, as in pleurisy, peripneumony, and phthisis pulmonalis. In bilious fevers it is often favourable; in typhous, nervous, and putrid fevers, it is on the contrary more to be dreaded. It is seldom critical in the fevers of this climate, whatever we may read to the contrary in European authors. The same may be said of bloody diarrhæa, or a discharge of blood by stool. Of other hæmorrhages the same may be affirmed, although it has been supposed that the relief obtained from this spontaneous evacuation originally gave rise to blood letting. Perhaps the frequent use of the lancet, in this country, prevents many crises by hæmorrhage.

3. Of *Expectoration*. This does not usually prove fatal unless the lungs are injured, but this is by no means always the case. In 1814, in the month of January, I had a patient with typhoid pneumonia, which disease he also had two years before. This patient was a labouring man upwards of forty. His expectoration was bloody and frothy, and in the advanced stage he had hiccups. Thirteen days after I was called, in this second attack, the patient was considerably better, so that I thought my further attendance unnescessary. Three days after this, I was called again, on account of a most inordinate expectoration of mucous matter brought up by hawking, to a quantity exceeding half a gallon a day. It was accompanied with a rising in the throat and a difficulty of deglutition. A powder containing half a grain of opium, combined with ipecacuanha and Armenian bole, repeated once in four hours, speedily subdued this formidable evacuation, which from the sequel, I learned gave more alarm than necessary.

4. *Puking* or *belching* a dark green matter is a very dangerous symptom; when matter of this kind is brought up by eructation or belching, the patient commonly dies. The matter of black vomit is, I believe, in the worst cases thus expelled.

Expectoration of black matters by coughing or hawking, has happened in the epidemick pneumonies, or lung fevers of late years, and not always proved fatal.

This black colour may be owing to extravasated blood; but I rather suspect it to be a peculiar secretion, and like the black colour of the lungs in aged subjects, to be unaccounted for.

5. *Stools* of a black, bottle-green, or sea-green colour, or those resembling a mixture of soot and sulphur, denote a severe disease, and it is remarkable, that such stools occur in the diseases of this country, whose other symptoms are very various and even opposite; as in dysentery, yellow fever, pneumony, angina, &c.

As relates simply to the stools, prognostick, however, is much the same in all cases; for if they approach a more natural appearance after a few cathartics, the result may be favourable; whereas if after frequent cathartics they appear equally morbid, and the strength fails, the patient may be viewed as having the whole contents of the primæ viæ vitiated, and is in very great danger. Stools which are entirely void of smell are perhaps the worst of all. It is one of the astonishing powers of a diseased system, to be able entirely to suppress so common a phenomenon as the fetor, and even smell of the fæces.

6. *Hæmorrhages.* These occur rather more frequently in the fevers of this climate from the bowels than from any other emunctory; but have not usually proved fatal since the method of treating them with acetas plumbi and opium has been adopted. I however saw one case of fatality from the evacuation in the patient of another physician, who did not use this remedy. Intestinal hæmorrhage in cases of colick is apt to end fatally.

Uterine hæmorrhage I formerly considered very dangerous, but I have never known it to end fatally. Dr. Zimmermann tells us, that in acute diseases it is a good sign, which is contrary to the general opinion. In one case, after a lingering anomalous fever, a slight hæmorrhage happened from the nose, upon which the appetite returned and the febrile symptoms abated. I saw my patient, a young lady aged 24, in the evening, October 1811, and thought her better, and felt myself happy in her favourable situation, as her case had caused considerable solicitude. The next morning, before the usual time of rising, I was called on to visit her. A hæmorrhage had commenced from the mouth, nose, uterus and bowels, and under the skin, shewing itself in spots and blotches of a claret and dark colour.

This universal hæmorrhage resisted all the remedies which could be devised to check it, and carried off the patient. I know of no method by which this unhappy and unexpected event could have been prognosticated, as the evening before the hæmorrhage, the patient had a good soft pulse of no inordinate quickness; her skin was soft, and her appetite returning.

7. Dark coloured *urine* threatens mortification, but has not always proved fatal. By Dr. Zimmermann we are told, that the colour of the urine may be white, pale, yellow, red, brown, green, and even black. He quotes one Plancus, as saying that he had a patient whose urine deposited a blue sediment; but as Plancus' patient was opened after death, and nothing of this blue colour discovered in the bladder, it was suspected that he made use of a copper chamber-pot. Few physicians in this country will agree with the celebrated Boerhaave, that the urine of patients, who die of acute fevers, is always colourless.

When we consider the vast difference in the urine of healthy persons, and that in some it is of a deep yellow colour all the time, that in some it turns milky soon after it is evacuated, and finally, that its healthy colour in some is its diseased colour in others, we shall have little reason to rely much on the urine alone as a sign of diseases.

It is a very striking instance of the secretory powers of human glands that a real grained sugar has been extracted from the urine of diabetick patients.

We are told by a celebrated European writer, that when limpid urine becomes turbid soon after it is voided, we may augur that nature is labouring to effect a crisis, which is, I believe, confirmed by the observations of physicians in this country.

Urine, which continues limpid, deposits no sediment, nor has any scum, denotes the fever to be severe, and the crisis distant.

According to Boerhaave, a white, inodorous, light, uniform sediment, is a good sign, both of the present and future: and to this, Zimmermann adds a minute particular, which I believe experience will justify, viz. that if this sediment, instead of being flat, is a little pyramidical, the sign is still better. Lead coloured urine is considered dangerous. The opinion once entertained of consumptive patients having oil float on their urine, is now justly esteemed fallacious.

I have seen a case related from Baglivi, of a patient, who laboured under some disease of the joints, and who after voiding a great quantity of turbid urine, which very soon became of a gelatinous consistence, recovered. It will be recollected that cantharides produce this effect, when taken in considerable quantity in form of tincture, as I particularly recollect in the case of a patient in the Pennsylvania hospital; which may suggest the propriety of using them in gouty, rheumatic, and white swelling cases.

The imposition which is practised by certain quacks, of pretending to tell all diseases, and to point out their remedies by the *urine*, is said to have originated in the middle ages, when the greater part of physicians were ecclesiastics, who either saw their patients in the churches, or were satisfied with inspecting their urine.

Zimmermann tells us a pleasant story of a man of merit, who having declared in favour of one of these water-doctors, began to praise him in all companies: a waggish student told him one day soon afterwards, that his new favourite was one of the cleverest fellows in the world, having predicted, by the urine of a cat, that there would be no more mice for a twelve-month. Sorely piqued at this raillery, he returned home, and preparing a mixture of urine, coloured with saffron, with some chalk for a sediment, he sent it by his servant to the urine doctor; the latter received his fee, and sent back his opinion in writing. The good man now acknowledged the imposition.

A *scum* on the urine, or a flocculent appearance half way down the glass, without sediment, denotes a slow recovery.

An expression of desire to evacuate the urine is sometimes the immediate forerunner of death, as I once saw in the case of a celebrated physician, whom I was called to see in the last stage of yellow fever. I was not at that time apprized of its danger, nor were the physicians in attendance in utter despair of the case; but a brother of the sick physician, who was present, and had paid much attention to sick persons, upon hearing it, pronounced him dying, and his prediction was confirmed by his breathing his last in a few minutes. It is thus that by close attention to nurses and attendants, much may be learned, by those not above drawing knowledge from such humble sources.

Copious, pale, watery urine, is a symptom of nervous affections. Urine abundant in quantity, and frothy, has been noticed in dangerous cases of spotted fever. The urine, in cases of mortification happening in France, from living on bad rye, is said to be clear.

Urine scanty and high coloured, is a sign of inflammatory diseases.

When the urine is entirely suppressed in fever, it is probable that the violence of the disease stops its secretion, or, on the other hand, that it renders the bladder so torpid as that the patient becomes insensible to its stimulus. I have suspected, on other occasions, that the stimulus of the urine, is, from the deranged sensations of the patient, mistaken for something else extraneous to the body, as the pressure of the bed-clothes, or some injury offered by the attendants. Hence, when the catheter becomes the dernier resort in fever, the patient is, I believe, apt to die; not by any means because it is used, but because the disease is very violent to render such a resort necessary.

8. *Obstinate costiveness* seldom proves fatal in fever, and it merits consideration, that patients seldom die with this state of the bowels. I have known it of fifteen days continuance. Dr. Rush mentions the case of a sailor who went to Lisbon and back without evacuating his fæces.

Perhaps our solicitude is apt to be too great for patients in this situation. Death, it is suspected, has been sometimes occasioned by over officiousness; especially when large repeated tobacco injections have been used.

9. *Spontaneous salivation* is favourable, both for cure and prevention of fever. During my attendance on a great many cases of fevers at once, I have sometimes spat as freely as though I had been slightly salivated, and which has, as I have supposed, prevented an attack of fever in myself. This spitting is not attended with any tenderness of the gums.

10. *Sweats.* These when profuse, even so as to wet the bed-clothes, are sometimes followed by an immediate cessation of fever and a quick recovery; but in the spotted fever, of late years, we have heard of people dying under profuse artificial sweats.

Cold sweats are unfavourable, but not always fatal. Sweats are said to be more influenced by the state of the stomach than by the pulse; but we must consider, that the state of the stomach influences both the pulse and skin.

Partial sweats about the neck and head augur ill; as do those sweats, if more extensive, which begin with the disease.

Clammy-sweats are the secretion of death; and are one of the many instances of the surprising powers of the secretory organs over the fluids.

11. OF VOMITING. Vomiting is sometimes pertinacious without any thing being thrown up, either in quantity or quality, which can lead to a suspicion of the cause. This kind of vomiting is sometimes relieved by an emetick; and it is truly

wonderful that an emetick will relieve hiccups, as well as vomiting, and other violent symptoms without evacuating any thing apparently offensive, either in quantity or quality.

Vomiting with a tense pulse, which is pertinacious, denotes an affection of the brain and calls for bleeding and blisters.

A violent attack of various and opposite diseases in this country, begins with vomiting, such as bilious fever, spotted fever, canker rash, measles, &c.

Dr. Zimmermann tells us, what our own experience will confirm, that vomiting is a dangerous symptom in pleurisy when it appears early. He would treat it by repeating bleeding.

New diseases have new motions of the blood vessels, and in consequence, of the glands; hence new secretions.

In spotted fever and similar ones prevailing since the year 1806, a matter has been thrown up resembling bilge-water, or the muddy water of swamps, which has usually been a fatal sign. In one case a claret coloured liquor, with a filthy substance, was thrown up with a favourable result, in two other cases a matter exactly resembling blue dye was in large quantities vomited, both of which terminated favourably. In other cases equally favourable the ejected matters have resembled water coloured with blue vitriol. These are instances of the surprising power of the secretory organs over the fluids. European physicians have supposed the matter of black vomit to be extravasated blood, but of this I have great doubts, as particles of blood would in that case be mixed with it, of which I have never heard of an instance; and no suspicion can be entertained of blood producing the blue colours just mentioned. Other reasons against this opinion are, that the bilge-water black vomit is too thin for blood, and the mucous black vomit too tenacious; and again, that the vomiting in hæmatemesis does not resemble either of the kinds of matter which we have mentioned. It may be in conclusion observed, that like spots on the skin, the darker the ejected matter, the worse the omen.

V. *Of the Senses.*

1. It is commonly supposed, that those who think that they shall certainly die when sick, generally do die; but this I have not found uniformly true, although despondency is very unfavourable.

Some patients of this description will have a service done them by raillery, and others by serious remonstrances upon their pretensions to foreknowledge.

2. Low delirium, with heedlessness and a locking up of the senses, is very apt to end fatally; simple deafness is far less dangerous.

3. Those who call out to have something taken off of their head, when there is nothing to remove, are in great danger; as are those who complain of some one having struck them on the head.

4. *Blindness* is a symptom sometimes recovered of, as we are assured; but in my own practice, when it has ushered in a disease, it has been usually a fatal symptom. In 1814, a man who felt somewhat unwell came to my house and was bled. He was taken blind on his way home, and was unable to find his house without assistance. The prevalent ataxic fever, generally called spotted, probably had been insidiously forming in his system for sometime previous. He lived about two weeks and died, never having regained his sight but partially.

5. A failing of all the faculties, and slight fatuity, is usually fatal.

6. A sharpness of expression, and extreme irritability of temper, are usually thought to denote recovery, but I have found them, both in adults and children, to end fatally.

7. A constant groaning, when no particular pain or distress can be pointed out, denotes such a load of disease and derangement of the vital organs, as is seldom recovered of.

8. Tears standing in the eyes or running down the cheeks, without any attempt to wipe them away, are a bad omen; as is omitting to brush off a fly, or other extraneous substance from the face.

9. An entire unconcernedness about recovery, and the patient saying he feels better and shall soon be well, when every thing bespeaks imminent danger, is generally a fatal sign.

10. Muttering and not noticing external objects, nor discriminating between things, exposed to the senses, from those imagined, are highly dangerous.

11. An expression of extreme happiness upon tasting any thing, or on having a pillow altered, or any similar trifle done, are forerunners of approaching dissolution: and in general it may be said, that the greater the variation in these respects from health, so much the worse, and the less so, so much the better; hence an acquaintance with our patients, their habits and modes of expression, are useful in forming a prognostick.

12. Great elevation of the faculties, especially in children, betokens dissolution. But typhus fever has sometimes appeared to sharpen the genius of adults without an unfavourable issue.

I knew a young man, who, during his convalescence from typhus fever, laid in his mind the foundation of a loom to go by water to weave narrow stuffs and cloth, which upon his recovery he carried into effect, and obtained a patent for, from the United States. I have had the pleasure of seeing it in operation, and think it does honour to Mr. Williams, and the state in which he lives.

Agreeably to the remark of Zimmermann, this elevation of mind is greater in young and middle aged persons, than in the old. He remarks, that, "It would seem as if nature carried these subjects through all the periods of life, as it were in a moment." See Exp. in Physick, vol. I. p. 336.

13. A numbness of the arms and legs has been complained of in cases of spotted fever, and its satellites, and denoted a severe and often mortal disease.

VI. *Of debility.*

1. There is a point of debility, depression or exhaustion in diseases, to which if the patient sinks, although he may revive from, yet he never recovers; and this is strictly speaking a death point: for although several days may intervene, or even a week, before it actually takes place, during which time many flattering appearances may occur, and hopes of recovery be entertained, yet the patient ultimately sinks to a similar state, from which no possible method yet discovered will arouse him. An erroneous prognostick may be given by a sagacious physician, who is called at this intervening period, from his not knowing the previous state of the patient.

2. There is much care required, to distinguish between the debility of oppression, and the debility of exhaustion. The first is like a wheel loaded with ice, which the water cannot move till it is unloaded; the second, like the same wheel clear of ice, but which does not move because there is not water enough to carry it.

In the first case, what the physician has to do, is to unload the wheel; in the second to add more water. In the former state of the system, indicated by the icy wheel, bloodletting, cathartics and emetics, are the grand remedies to restore action and energy; in the second, wine, laudanum and other stimulants. But these two states of the system are by no means so distinct and well defined, as it is easy to make them appear on paper. Nor are they the only states of the system which occur, for the same diseased body may be plus in one part, and minus in another, and hence may require repletion to one set of organs, and depletion from another, may in fact require wine and laudanum to the stomach, and evacuations from the blood-vessels. But it remains for men, whose sense and abilities are far superiour to those of mankind in general, to comprehend, how these two states of the system are both to be relieved by the same remedy; and this remedy a single, sharp pointed instrument: or to cease to wonder, why nature has provided such a profusion of remedies, when so very few were needed; or that lancets were not among the number, when so universally required!

We are, for instance, directed to bleed, because the pulse is low, to make it stronger: well, and what then? Why to bleed again till we make it weaker. But supposing, from any cause, that the pulse becomes imperceptible? Why then we are directed to bleed again to bring back the healthy beat!

Let me not be accused of exageration; the reader will find every thing here asserted, and more, in the celebrated "defence of blood-letting."

It must be kept in mind, that the author of this system explicitly denies debility to be a disease. We are aware of such things as "fears of the brave, and follies of the wise," but it would seem hard to find an instance of error so palpable, in a character at once so very great and amiable. Let me by no means, whilst I call in question a part of the doctrines of the late venerable Dr. Rush, be ever thought insensible to his worth, or the general usefulness of his writings. Those who have made themselves acquainted with the particulars of this system, need not be told, that disease is considered as a unit, and therefore but one remedy is needed. Again, diseases are to be *cured;* a modern and unlicensed term, for, as Dr. MITCHILL observes, who ever heard of curing small-pox? All that was formerly attempted, was the good old Hippocratic method of watching and moderating the symptoms, until nature cured the disease; but *nature,* we are expressly told, is to be treated like a noisy cat in a sick room,—to be thrown out; I use his own expression, vid. M. S. Lectures.

VII. *Prognosticks by the pulse.*

1. By Hippocrates, the pulse is said not to have been mentioned at all, whilst in the practice of modern physicians, and especially since the discovery of the circulation of the blood, it is deemed to the physician, what the mariner's compass is to the mariner.

The American Hippocrates, Dr. Rush, gave to the pulse a very uncommon share of attention; his divisions and descriptions of it being embarrassingly minute.

2. It seems that the same pulse has a different prognostick attached to it in different countries. The dicrotos pulse (from two Greek words signifying *twice* and to *strike*) is said first to have been observed by Dr. Solano, who considered it a certain sign of approaching critical hæmorrhage from the nose; but it has not, so far as I have observed, been followed by that evacuation here.

3. A pulse habitually irregular may become regular by disease; and then the usual irregularity will be a sign of returning health.

4. Weakness and slowness of the pulse, joined with local pain, indicate spasm, as is rightly observed by Dr. Zimmermann.

5. By the quickening of the pulse alone, we may be led to foretell a severe disease at first, and the approach of death in diseases already advanced. Some exceptions as in most other cases will occur, for I lately had an adult lady under my care, whose pulse was 150 in a minute, who yet recovered.

6. A pulse which is extremely irregular does not always denote a fatal termination, as I have learned by experience, having formerly from my readings entertained a somewhat different opinion.

7. A low, quick, weak, tremulous, irregular pulse, must be considering as denoting fatality with much certainty; as also an absent pulse, unless in cases of swooning.

8. Every new epidemick will probably present some variety in the pulse. The modern ataxic fever, called spotted, and by other names, in some instances presented a pulse which conveyed the idea that the artery was filled with milk, water, or some thinner substance than blood. Other cases, which were lingering, had a pulse which conveyed a sensation of fulness without resistance, as though the blood had undergone a fermentation, and generated yeast or elastic vapour; the latter occurred in elderly people, and pointed to a fatal termination under every mode of treatment.

This is perhaps the kind of pulse denominated gaseous by Dr. Rush. In some other cases the pulse presented the idea, that a column of something different from blood occasionally reached the heart, entirely unfit to stimulate it to a healthy action; hence a fluttering of the pulse, faintness and paleness.

9. A violent throbbing of a particular branch or branches of an artery, whilst the other arteries are not excited, is a

very unfavourable sign; this excitement is most apt to happen in the carotids, in which the symptom is not quite so bad, but the danger is increased when it happens in the arteries more distant from the head. A fatal case, in the patient of another physician, fell under my notice, in which this throbbing was in the anterior tibial artery. When it is universal (by reason that all the branches bear an equal share of the burthen) it is not so alarming. In one case of a child, this throbbing was so mightily increased, as to give a slight motion to the cradle in which it lay, which yet recovered. I was informed of another case, however, which, with symptoms somewhat similar, in an adult, proved fatal.

10. Since the appearance of spotted fever in 1806, as an epidemick, this throbbing of the arteries has been a common phenomenon. In a man 27 years old, to whom I was called in the last stage of his illness, at the approach of death, the carotid arteries exceeded every case of which I had formed any conception. In a note to a letter sent to Dr. Rush, I have noticed the following particulars of this case. "In one uncommon case, the carotids appeared distended to the bigness of a man's thumb, with exquisite throbbing; after death the face cracked open on each side the nose."

This man's illness began in the small of his back, with a lameness rather than pain, but which early in the attack disabled him from walking. He had spots on his face resembling the oblique punctures of a needle. It happened in July 1812.

11. I have before hinted, that a violent disease is to be apprehended from a very quick pulse at the first attack. Two cases are recorded in my journal, illustrative of this remark; one of them proved fatal, the other recovered with much difficulty. They both excited much anxiety, being only sons.

The last case occurred in February 1815, in a boy about eight years old, who had been at school on the day of the attack. I found his pulse 139 in a minute upon first examining it, and felt the mortality of the case under my fingers.* He lived ten days, and during his illness evacuated pints of matter, by stool of a black uniform colour and consistence, without any appearance of natural fæces, and of the thickness of blue dye. In his last moments, some of this matter, I was informed, came out of his mouth and nostrils; shewing the contents of the whole primæ viæ to have become vitiated.

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* His parents were first cousins.

It is remarkable, that in this case the evacuations were entirely void of fetor, and nearly void of smell. Physicians must ascertain this for themselves; for I have scarcely known it fail, that, if a stool looked unusually bad, the attendants would not report it to be very offensive to the smell, which is far from being always the case.

During the prevalence of spotted fever and its congeners, very surprizing instances of death have happened in families, of individuals, who had scarce any perceptible illness. And, in some instances, such deaths have happened about the same period that another member of the family, who had been extremely ill, began to recover. It seemed as if the disease was transferred from one to the other in its advanced stage, and that it might be said, that one member of the family had been extremely ill and recovered, whilst another had died with very little preceding illness. Such cases will bid defiance to all prognosticating, unless very accurate attention be paid to the pulse, which sometimes, by its quickness, gives a clue to the impending danger.

VIII. *Of the tongue and taste.*

1. A *white* tongue is said by Dr. Rush to denote inflammation, but this does not appear to be always the case, as cases of fever have happened, with a white tongue at first, which has been followed in a day or two by one of a dark colour, while the other symptoms have denoted a state of the system, the reverse of inflammatory from the first, and the patient has recovered without any antiphlogistic remedy.

2. A *yellow* tongue denotes an increased secretion of bile, and calls for emetics.

3. A very *red* tongue, without fur, denotes an irregular fever without crisis. This kind of tongue sometimes occurs originally, and sometimes follows a furred one. If attended with dryness, denotes a slow recovery, with irregular and perplexing symptoms.

4. A *cracked*, scaly and dry tongue, denotes much the same as the last mentioned: viz. a fever without crisis, and followed by relapses.

5. A *black* tongue, is usually thought to denote a putrid fever; this I suspect however is far from being always the case. It is perhaps oftener that a black tongue is owing to black bile, and has no connexion with putrescency. A tongue of this colour, however, may be generally considered as a

mark of the typhoid diathesis, and seldom if ever of the inflammatory.

6. A bitter, saltish, or acid taste, in the mouth, denotes a foulness of the stomach, and calls for emeticks. The two latter accompany nervous diseases, perhaps more commonly than fevers.

7. A fetid taste in the mouth, denotes a foulness of the stomach, or rather sometimes an abscess of the lungs or other viscera. Or, on other occasions, nothing more than simply a catarrhal affection, in which the retained mucus has become thickened and vitiated.

8. A tongue covered entirely with fur to the very tip and sides, denotes such a load of disease as is seldom recovered of.

IX. *Of the temperature of the surface.*

1. Extreme heat in fever may be considered dangerous, and is rather a symptom of the inflammatory than typhoid diathesis, although this cannot be considered as always the case.

2. The experience of late years has verified a remark made in Europe, that the worst cases of fevers, and even the plague, attack with coldness instead of heat.

3. The redness of inflammation, is, I believe, always accompanied with heat; but there is a redness, especially of the cheeks, which has no increase of temperature, being probably the redness of effusion, accompanied with a feeble pulse and other marks of direct debility. Spectators, who confound redness with heat cannot be convinced to the contrary, unless they are brought to the bed side, and made to touch the patient's face themselves. This I have sometimes prevailed on them to do in order to increase their confidence in the mode of treatment.

Note. It is proper to remark, that some of the prognosticks mentioned above, are as applicable to fevers of other places, as to those of New-England.

Observations on the Proofs of Murder by Poison. By T. Romeyne Beck, M. D.

[Communicated for the New-England Journal of Medicine, &c.]

AN article in the New-York Medical Repository has lately attracted my attention, and, after giving it several perusals its contents have appeared to me imperiously to demand some animadversion. I submit my remarks on it to the Editors of the New-England Journal, for insertion in their publication, provided they may judge them worthy.

The article to which I refer is contained in the Medical Repository for July 1816, p. 287; and is entitled "*A Case of perforation of the stomach, obstructed by a large tumour and affording power of digestion.* By Felix Pascalis, M. D. *Read before the Medical Society of the City and County of New-York, at their anniversary meeting, July 1st.* 1816. The Remarks which precede the history of this Case are the subject, to which I wish to call the attention of your readers. They appear to me to lay down incorrect rules in Medical jurisprudence, and to give a most extraordinary mistatement of a medico-legal case. I shall notice each of these in their order.

1. Dr. Pascalis, after making some observations on the discovery of John Hunter, that the gastric fluid, in certain cases, corrodes the stomach after death, remarks, *that it is a rule now sufficiently established in Medical jurisprudence, never to admit the perforation of the stomach as a proof of Murder by Poison.* I was, I confess, not a little startled at this broad assertion, and although I am as unwilling as Dr. Pascalis possibly can be, to admit perforation of the stomach to be a conclusive proof of murder by poison, distinct from other appearances, yet I was not aware, that it was excluded from the list of signs found on the dead body. Dr. Pascalis however says, that such is the rule in Medical Jurisprudence. If so, we shall undoubtedly find it in the writings of Authors on that science, and I have been induced to examine those in my possession, for the purpose of ascertaining their opinion. The result is as follows:—Mahon observes "La roideur des membres, et la tumefaction du ventre, citées par quelques auteurs comme des signes d'empoisonnement, quand elles ont lieu aussi-tôt après la mort, ne sont pas des signes constans. Mais ce qu'il y a de constant dans les cadavres des personnes qui ont péris d'un poison âcre ou caustique, c'est de trouver l'œsophage, *l'estomac*, et les intestins grêles atténués, enflammés, gangrenés, *rouges et souvent percés.*" (Médicine légale et Police Médicale, de P. A. O. Mahon, &c. Paris, 1811, vol. 2, p. 308. Foderé makes similar observations, and in one place, has the following remarks, "Les poisons acres et narcotico-âcres peuvent quelquefois produire les mêmes effects que les corrosifs, et ces derniers operer des lesions differentes de celles *qu'ils produisent ordinarement*; ainsi M. Marc ajoute en note a

traduction du manuel de M. Rose, *qu'au lieu de l'etat d'erosion des membranes de l'estomac*, il peut arriver de les trouver epaissies." (Traité De Médicine légale et D'Hygiène Publique, &c. par F. E. Foderé, M. D. Paris, 1813, vol. 4, p. 271.) Farr, in his directions about the examination of the stomachs of persons supposed to be poisoned, mentions as one of the circumstances to be noticed "*Whether it be perforated into holes, either one or many*; and after mentioning several others, he concludes by remarking, "*All these marks afford very strong suspicions of poisons, especially of those which are acute and acrid.*" (Elements of Medical Jurisprudence, by Samuel Farr, M. D, 2d. edit. London, 1814, p. 89 and 90.) Orfila, speaking of Corrosive poisons, says, "Inflammation of the first passages, constrictions of the intestinal canal, gangrene, sphacelus, *perforations of the parts*, constitute the first character of these kinds of lesions. We could bring forward, in support of this proposition, the results of dissections of several animals, which we have poisoned with different substances of this nature," (a general System of Toxicology, by M. P. Orfila, M. D. London, 1816, p. 15.) Again, when speaking of Corrosive sublimate, he states, that Sallin denies its producing perforation of the intestinal tube, but adds, "The assertion of Sallin does not appear to us admissible. By experiments made "upon animals and a collection of cases of poisoning stated "with great care, it is proved in a manner incontestable, 1st, "*That a general inflammation of the Alimentary Canal, as "well as its* PERFORATION, *can be produced by all the corro"sives, &c.*" (Pages 61 and 62.) In another place, Dr. Orfila enumerates among the lesions of texture induced by the arsenious acid, "gangrenous spots on the stomach and duode"num, sloughs, *perforations of their coats*," &c. (P. 127.)

2. Dr. Pascalis quotes from the Paris Journal of Medicine, a case of sudden death, occuring after an illness of seven hours. On dissection, the stomach was found perforated in two places, but no vestige of any deleterious substance could be discovered, "*nor was it probable*," he adds, "*that had any persons made "an attempt against her life, as seven hours only had elapsed "from the time of her appearing in her ordinary state of health.*" I do not mean to question the correctness of the opinion concerning the cause of death, in this case, but if I understand the Doctor's concluding remark, it is certainly a dangerous one to be introduced into Medical Jurisprudence. The common sense of mankind, and indeed of the profession, most undoubtedly would lead them to consider the giving of poison probable, or at least NOT IMPROBABLE, when a person, in ordinary

health, should die after seven hours illness, and particularly if the causes that produced death were not clear and manifest. Such a suspicion I should deem just and laudable, and in proportion to the previous degree of health, ought to be the alarm excited from the sudden extinction of life. Dr. Pascalis proceeds to relate the following Medico-Legal case.

"3. A remarkable case came before the Assizes of Liver-"pool five or six years ago: it was that of a woman who died "suddenly in a boarding house, where she was supposed to "have been poisoned with a view of procuring abortion. Dur-"ing and after the Coroner's inquest, inconsiderate and pre-"mature impressions were authorized by many eminent physi-"cians, and directed against the keeper of the house; but what "is more to be regretted to have taken place in an enlighten-"ed state of society, the perforated stomach of that woman "and her diseased uterus had been handed about from Liver-"pool to London and back again, for the express purpose of "collecting among medical men, authorities enough to sup-"port the charge against a murderer."

"The defendant was of course presumed to be the perpe-"trator of it, and he would surely have been convicted, had "not Dr. Carson of the city of Liverpool, (a name to be held "up with pleasure and respect) by an eloquent demonstration "to the Court, proved that the autopsic report of the Coro-"ner, signed by so many celebrated names, was grossly de-"ficient, and that a spontaneous disease was far more proba-"bly the cause of the appearances on the stomach and uterus, "than any poisonous ingredient. An innocent victim of er-"ror and prejudice was thus wrested from an ignominious death "by the help of medical science."—Such is the history given by Dr. Pascalis, of the famous trial of Charles Angus, Esqr. for the murder of Miss Burns. Permit me to examine it in detail: 1. "*A remarkable case came before the assizes of Li-" verpool, five or six years ago.*" The case came before the assizes at *Lancaster* in Sept. 1808, (Edinburgh Med. and Surg. Journal, vol. 5, p. 220 and 225.) 2. "*It was that of a " woman who died suddenly in a boarding house, where she " was supposed to have been poisoned, with a view of procur-" ing abortion.*" I cannot find any thing warranting the statement, that Mr. Angus kept a boarding house, in the writings that I have consulted. Dr. Pascalis is however very probably correct. But the following circumstances should have been added, Mr. Angus had been married to the sister of the deceased, and after the death of his wife, which took place three years previous, Miss Burns had lived with him as his housekeeper, and governess of his children, and continued to do so, until the time of her death. She was ill about fifty hours, and during that time refused medical assistance. (London Med. and Phys. Journal, vol. 21, p. 336, &c. and Edinburgh Annual Register, vol. 1, part 2, p. 188.)

3. "*During and after the Coroner's inquest, inconsiderate* "*and premature impressions were authorised by many emi-* "*nent physicians, and directed against the keeper of the* "*House.*" In answer to this statement, the following circumstances may be mentioned. On the 27th of March, 1808, Dr. Rutter was desired by the Coroner of Liverpool, to take with him an experienced surgeon to the house of Mr. Charles Angus, there to examine the body of a young lady who had died suddenly. On opening the abdomen, some fluid was found to have been effused into that cavity, of a yellowish colour, and turbid. Marks of inflammation were found on the external or peritoneal coat of different portions of the small intestines, but the large intestines were free from it. The external coat of a part of the smaller curvature of the stomach was also inflamed, and a similar appearance of inflammation was observed on a small portion of the anterior edge of the liver, directly over the smaller curvature of the stomach. On raising up the stomach, an opening through its coats was found in the anterior and inferior part of its curvature, and from this opening, a considerable quantity of a thick fluid of a dark olive colour issued. Some ounces of this fluid were collected and preserved. The natural structure of the coats of the stomach for a considerable space around this opening was destroyed, and they were so soft, pulpy and tender, that they tore with the slightest touch. Around this part of the coats of the stomach, there were no traces of inflammation whatever. On examining the womb, it was found to be very considerably enlarged, and on its inner surface, the part to which the placenta had adhered, was very plainly discernible. This part was nearly circular, and occupied a space of about four inches in diameter, the mouth of the womb was greatly dilated. In a word (say Dr. Rutter, and Mr. Christian, the surgeon, in their Report) the appearances of the womb were such as might have been expected a few hours after the birth of a child nearly full grown. The fluid, taken out of the stomach, was examined by Dr. Bostock, an eminent Chemist, but no poisonous substances could be detected by him.—He then poisoned several dogs with corrosive sublimate, and examined the fluid found in their stomachs, but was unable to detect the poison. Let it be recollected that this was done between the 17th and

22nd of August, and the trial did not come on until Sept. (Edinburgh Med. and Surg. Journal, vol. 5, p. 14. See also a case by Dr. William Henry of Manchester, in which he states the examination of the fluid found in the stomach of a young woman, who confessed that she had poisoned herself with corrosive sublimate. The poison could not be detected by any test. Ibid. vol. 7, page 150.) From the above circumstances, the medical gentlemen (Drs. Bostock, Gerard and Rutter, and Mr. Hay and Mr. Christian, surgeons) swore that they believed Miss Burns to have been poisoned. The Reviewer in the Edin. Med. and Surg. Journal, who is known to be Andrew Duncan, jun. M. D. Professor of Medical Jurisprudence in the university of Edinburgh, after reviewing the trial, and the observations of Dr. Carson, says, "upon the whole, "the suddenness of the attack, the nature of the symptoms, "and the appearances on dissection, rather lead us to believe "that Miss Burns *was killed by poison.*" Surely it requires some boldness to accuse these men of *authorizing inconsiderable and premature impressions*, after examining the above report.—If they thought Miss Burns had been poisoned, were they not right in telling the Coroner so? They might be mistaken, but surely the above circumstances were sufficiently unusual, to demand further examination, and I may ask Dr. Pascalis, to point out to me the kind of Medico-legal cases, that require legal investigation, if this is not one? In order to prevent repetition, I will state here, that my authorities concerning the trial of Mr. Angus, and the controversy between the medical witnesses for the crown and Dr. Carson, are the Edinburgh M. and S. Journal, vol. 5, p. 220, London, Med. and Phys. Journal, vol. 21, p. 336 and 426, and the Edinburgh Annual Register, vol. 1, part 2, p. 188.

4. "*But what is more to be regretted to have taken place in an enlightened state of society, the perforated stomach of that woman, and her diseased uterus, had been handed about from Liverpool to London and back again, for the express purpose of collecting among medical men, authorities enough to support the charge against a murderer.*" The gentlemen, who examined the body, were so firmly convinced, from the appearances discovered on dissecting the uterus, that Miss Burns had been recently delivered of a fœtus; that they did not conceive it could be questioned, until Dr. Carson gave his evidence. He was of opinion, that the uterus of the deceased might have been distended by a collection of water, or rather hydatids; and that the mark which the other gentlemen supposed to have been occasioned by the attachment of a placenta,

was occasioned by the attachment of hydatids. The diversity of opinion, that thus existed among the medical witnesses, was the cause of the acquittal of Mr. Angus, but the uterus having been preserved, was again examined on the 14th of September (twelve days after the trial.) The *ovaria* were then for the first time divided, and a *corpus luteum* was distinctly perceived in one of them. The medical witnesses for the crown, stated this in their "Vindication." Dr. Carson, in his "Reply," denied its being a proof of previous pregnancy. THEN, AND NOT TILL THEN, did Mr. Hay take the uterus up to London. He shewed it to Drs. Denman and Haighton, Messrs. Cline, Astley Cooper, Abernethy and C. M. Clarke, and the unreserved opinion of each, given severally and distinctly, was, that they knew of no means of accounting for the appearance, but by an advanced state of pregnancy in the deceased, (London Med. and Phys. Journal, vol. 21, p. 344.) Is there any thing inconsistent in this conduct, with what belongs to an *enlightened state of society?* If the medical gentlemen in Liverpool could not agree on this point, was it not proper, to inquire of the first surgeons and accoucheurs, in this kingdom, what their opinion was concerning it? And above all, if one man accuses another of murder, is he right in adducing all the testimony in his possession, and in placing it in a true light before the public?

5. "*The defendant was of course presumed to be the perpetrator of it, and he would surely have been convicted, had not Dr. Carson, of the city of Liverpool (a name to be held up with pleasure and respect) by an eloquent demonstration to the Court, proved that the autopsic report, signed by so many celebrated names, was grossly deficient.*" I can hardly agree with Dr. Pascalis, in speaking so highly of Dr. Carson. His conduct both before and during the trial, was not such, as ought to receive respect. He had never examined the diseased parts, and, just before the trial, he told one of the medical witnesses for the crown, that he did not know why the defendant had subpœned him to Lancaster, as he knew nothing about the case. (London M. and P. Journal, loc. Citat.) But he *proved by an eloquent demonstration* to the Court, that his views were correct? How does this agree with the following extract from the charge of the Judge to the Jury? "With respect to the circumstances of the deceased having been with child, you are put into the place of deciding where doctors disagree. You know it is a common observation, that is made in such dilemmas as these, 'who shall decide when doctors disagree,' and yet you are under the necessity of

making such a decision in the present case." (Edinburgh M. and S. Journal, vol. 5, p. 229.)

6. "*And that a spontaneous disease was far more probably the cause of the appearances on the stomach and uterus, than any poisonous ingredient.*" How correct this assertion is, as it respects the *uterus*, I have already shown. Dr. Carson dwelt much on John Hunter's observations concerning the disorganization of the *stomach* by the gastric juice, and had no objection to refer the appearances in Miss Burns to that cause. The subject itself is enveloped in considerable mystery, but it must certainly be denied, that the dissection agreed with the peculiarities mentioned by writers on disorganization of the stomach. To corroborate my remark, I refer to Baillie's Morbid Anatomy, and Dr. Yelloly's paper on the "*Vascular appearance of the human stomach, which is frequently mistaken for inflammation of that organ,*" in the Medico-Chirurgical Transactions, vol. fourth.

7. "*An innocent victim of error and prejudice was thus wrested from an ignominious death, by the help of medical science.*" As the doctors *disagreed* on the trial, Mr. Angus was acquitted. So far the Jury did right. But I cannot agree in believing the prisoner *innocent*. How far his guilt extended may be another question, as he was indicted on two counts. First, for poisoning the deceased; and second, for having given poison to cause an abortion. The decision is now with Mr. Angus and his Maker, but I may observe, that a more unfortunate case could not possibly be selected to prove the triumphal acquittal of an innocent man---a case, which the ablest medical jurists consider as proving the guilt of the accused, and which, every man who examines it, must, at least, consider a very doubtful one.

I have now gone through the observations, which I intended to make. I am not aware that I have misrepresented any of Dr. Pascalis' remarks, or examined them with unfairness. If I have, I am ready to apologize for it, as soon as it is pointed out. But until this is done, and until Dr. Pascalis shall prove the fallacy of my arguments or incorrectness in my quotations, I stand his accuser at the bar of the medical public, for having controverted fixed rules in medical jurisprudence, and for having incorrectly stated an important medico-legal case.

Albany, Feb. 4th, 1817.

On the Management of Infants. By J. G. Coffin, M. D.

[Communicated for the New-England Journal of Medicine, &c.]

THE object of these remarks is to discountenance the application of cold water to new-born infants. If the children who are born in cold climates could be so reared as to rise above the influence of severe and variable weather, they would acquire the highest attainable security for health and longevity.

This is the great purpose of physical education. But how is this desirable object to be accomplished?

Not, on the one hand, by an attempt to withdraw ourselves from those atmospheric changes which, in this country, we are destined to encounter; nor, on the other, by disregarding the effect of habit, to rush unprepared from one extreme of temperature to the other. We can triumph only by such frequent, *gradual*, and habitual contests with our enemy, as shall enable us either to evade or surmount his assaults.

But that I may assign a reason for rejecting the practice opposed, permit me, in a few words, to consider the nature and effects of cold water. Setting aside an examination of this agent, as a *remedy* in diseases, I know but two ways in which it can improve health or prevent sickness,—that is, in which it can be beneficially used by the well.

The first mode is, where cold is applied to counteract a degree of heat which is injurious from its excess; the second is, when it is so used, as to give strength,—to invigorate the feeble operations of a sound body. Presuming this view of the subject to be just, let me inquire, what effect the application of cold water to the bodies of new-born infants is likely to produce. It cannot act beneficially by diminishing excessive heat, for this does not exist. The question then occurs, can it be useful in the other mode of its operation?

Immersion in cold water, or its affusion over the body, is attended with three distinct effects,—an instantaneous and powerful shock, a sudden abstraction of heat from the surface of the body, and that exertion of vital energy to counteract the shock and to restore the lost heat, which is termed reaction. The practice therefore is never safe, pleasant, or salutary, unless it so affects the system as to be followed by this return of blood and warmth to the skin, with grateful sensations and sensible refreshment.

That these good effects may be secured, the water should be sufficiently cold, and extensively and expeditiously applied.

If these circumstances are disregarded, the effects, on the contrary, will be irksome and hazardous. Now the washing of infants with cold water is conducted in this manner. The nurse, as if apprehensive of doing mischief, though wedded to her error, applies the water partially and sparingly, here a little and there a little, but no where in sufficient quantity for the purposes of cleanliness or health, provided the fluid were of the proper temperature. This scanty application of the water has, I know, been called prudence, and distorted into an argument in favour of the practice;—but it ought to be called, what it is, an absurdity, because the water if suitably attempered, ought to be used *more freely*, and if not of the right temperature, it ought not to be used at all.

Important and desirable as it is that children, in northern climates, should be rendered hardy and familiar with cold air and cold water, it should never be forgotten, that this firmness and security can only be obtained by slow and cautious advances.

The washing and dressing of infants, with their necessary exposure to the air, constitute a sufficient commencement of the *seasoning* regimen for the first three months, during which the water should not be suffered to communicate any sensation of cold. The only exception to this rule is, where the child is oppressed with atmospheric heat, in which case the water should be *pleasantly* cool only.

From this period, if the infant be well, and the weather not cold, the temperature of the water may be gradually lowered, so that at the end of six months, if this be in summer, cold water may be used, that is to say, water not artificially heated. Shall the mother impose on her tender infant a severity, which she herself would shudder to encounter when in health?

What would the never-doubting nurse think, if the harsh expedient were prescribed for herself, which, with equal want of judgment and feeling, she practises on her helpless charge? There is no period of life, nor any condition of health in which this use of cold water is so unseasonable, so dangerous, and I may truly say, so cruel. That so abrupt a change as this is unseasonable or unnatural, will appear from a consideration of the operations and provisions of nature.

The freezing blasts of winter do not at once succeed the fervid heat of summer; the cooling breezes of autumn are interposed, to admonish the vegetable world of the decline of the year and the progress of the seasons.

Among the lower animals, also, the same precautions are observed. Birds protect and defend their tender young by their nests, the warmth of their bodies, and shelter of their wings. Other animals act on the same plan, in conformity to their different habits and varying resources.

It is for man only to disregard a principle, which we should think *reason* would easily recognise and promptly obey.

That this practice is full of danger to the wellbeing of the infant, will not so readily be admitted, because it will be said, that many children have done well under it. In reply, I contend, that its tendency is always unfavourable.

It is true, that many infants have indeed survived very improper and unkind treatment; but this surely is no defence or recommendation of such treatment, any more than the advanced age of certain intemperate and irregular individuals can recommend or justify their mode of life.

The infliction of useless pain is tyranny, and this practice wants nothing but a malignant intention to be such; while the injury is not the less severe on the innocent sufferer.

But so far as this custom arises from a degree of ignorance or prejudice, which might be removed by a disposition and efforts to obtain just views of the subject, it is blamable and cruel.

On the last day of January, 1816, I found a little patient, three days old, under the cold ablution. She had been previously well and quiet, her loud and piercing shrieks now drew me to her apartment. On ascertaining the cause of her outcries, I desired the nurse to make the water at least luke-warm for the future. Her self-complacency was evidently much disturbed, and her assumed prerogative almost unpardonably infringed.

She complied with the worst grace possible, declaring, that this was the second request only of the kind that had ever been made to her during a long period of many years' employment. This assumption of nurses is not exclusively their own fault, too many mothers consult them as oracles, and obey them with an entire surrender of their own judgment. This led me to inquire, how prevalent a custom might be, which I had rarely witnessed in twenty years' business, and which I had never approved.

Of seven accoucheurs whom I consulted in this town,

1, was indecisive,

1, left the matter to the mother and nurse,

2, used cold water soon after birth,

2, disapproved of it, and

1, said, that infants generally lived whether washed in warm or cold water, and seemed quite indifferent which was preferred.

4, Of an equal number of nurses, always used warm water unless desired to do otherwise,

3, When left to themselves, used cold water, after the first washing, without any regard to the season of the year. From this it appears, that physicians do not always do their duty, and that mothers and nurses are not to be trusted in this matter, without watching.

I do not find any writer on the diseases or management of infants who recommends this practice.

Buchan and Underwood, two of the best authors on this subject, speak of the practice in question, in terms of just reprehension.

I shall close these few remarks, which might be easily extended, by advising mothers to read Dr. William Buchan's "Advice to Mothers." This will, in some good measure, enable them to prevent the mischief which is daily arising from the negligence of physicians, the ignorance of parents, and the rashness of nurses.

Here my remarks ended, and I sent them to a medical friend to read; he has expressed a wish, that I had spared the censure on the nurses, observing, that they derive the practice in question from medical authority, and for himself, that he had not suspected any injury from it. No one is more inclined than I am fully to estimate the value of good nurses and to respect them, or more ready to believe that many such exist. Within the week past I have detected enough of hypocrisy in a worthless one, long to induce me to appreciate highly those who labour to understand and perform their duty.

I would blame and repress only that arrogance and selfishness which prompt some, much sooner to invade the province of the physician than to be useful in their own, rather to impose on the mother and neglect the infant, than to serve the one or take proper care of the other. I would persuade mothers, that if they will exercise their own understanding, they will be quite as likely to comprehend a matter of common sense, as the despotic leaders of the nursery, who in their turn, are too often guided by nothing better than the blind maxims of unthinking tradition.

However this chilling practice may have arisen, its effects, if injurious, as I apprehend them to be, are not the less to be regretted on that account. If, in truth, it has originated from a medical source, it would seem to be peculiarly proper that those guardians of the public health who think unfavourably of it, should expose and renounce it, that the improvements of each succeeding generation of physicians may repair, as far as possible, the defects of their predecessors.

With regard to those subjects which divide the profession, the individual cannot do better than to study them with an open mind, till he can discover the side of preference; to take his sentiments from the dictates of reason, observation, and experience, and to pursue that practice which directs him with the greatest certainty to the comfort and security of the well, and the restoration of the sick. It has been said, or I have imagined, that on some topics we do not rest on the line of truth till we have digressed from it in various directions, have perceived our distance from it, and have felt our need of its support.

If this be true, it only shows the advantage of entering early on the route of discovery, that we may arrive at certainty in good time to make our knowledge useful. The determination not to change, is becoming only to infallible minds. No opinion should be held too dear to be relinquished for a nearer approximation to truth, than the ground we may occupy. In the meantime the highest authority which any man can acknowledge as qualified to govern his conduct, must be those convictions of his own mind which he knows to be honestly, and which he believes to be correctly formed.

Case of an Extra-Uterine Fœtus, successfully extracted by the operation of Lithotomy, by JOSEPH BOSSUET, *member of the Medical Society of Massachusetts.*

[Communicated for the New-England Journal of Medicine, &c.]

IN the month of October, 1807, Mrs. Colman, of Braintree, found herself in a state of pregnancy, attended with uncommon distress, and some pungent pains, shooting from the hypogastric to the epigastric regions. She continued in that way, until the latter part of the ensuing spring, when she had all the symptoms of a true travail. She sent immediately for an accoucheur, who, not being able to come at the child by the natural passage, ordered large doses of opium, with the injunction to repeat them as often as the pains recurred. A fortnight after that, the pain abated---the time when I suppose the child died in the abdomen. She was, for two months afterwards, very much troubled by a disagreeable sensation, which she called *drawing*. The abdomen swelled to a very large size, which, after some time, gradually subsided. The three succeeding years she passed without much distress, but, at the end of that time, she began to experience very acute pains, attended with profuse evacuations, by the urethra, of a matter, sometimes of a yellow cast, sometimes bloody, and of a very fœtid smell; and voided in the same time, by the same canal, some very small bones. A communication also took place between the bladder and the rectum, so as to let the fæces and urine pass either way.

During five years before my first visit to her, she experienced the most excruciating pain, night and day. Having been informed of my recent arrival from Martinico, with my family, and that I resided in Hingham, she sent for me the 20th of May, 1816. I visited her the same day, and, after a critical examination, found the child lodged in the bladder, crusted over with a calculous matter. Considering her in a dangerous situation, I advised her to submit to the operation of lithotomy, as the only means to relieve her from her sufferings. She readily consented to it, and the operation was performed by me, the 17th of June, 1816, attended by Doctors Robert Thaxter, of Dorchester, and Noah Fyfield, of Weymouth, two respectable members of the Medical Society, in whose presence one hundred and forty-six bones of a fœtus, about seven months old, were extracted, together with a stone about the bigness of an olive.

She has since entirely recovered, and enjoys, at present, a perfect state of health, without any pain whatever: but the communication between the bladder and the rectum is not yet wholly obliterated.

Hingham, March 24th, 1817.

Arsenic.

[To the Editors of the New-England Medical Journal.]

Gentlemen,

DR. CHANNING favoured me, a few days since, with a piece of an ore from South America, supposed to be arsenic; its form appeared to be testaceous, colour externally hair brown, internally of a French grey, faintly glimmering lustre externally, shining metallic internally; brittle, hard, fracture fine grained; specific gravity 5·272. Totally volatilized by a gentle heat, and on examination proved to be pure arsenic.

The importance of being able, unequivocally to recognise this substance in cases of juridical investigation is well known, and no small degree of attention has been bestowed to discover new and infallible tests of its presence. The use of nitrate of silver, first proposed by Mr. Hume, and as improved by Dr. Marcet has been thought a complete and certain indication of arsenic; if we dissolve a little white oxyd of arsenic in distilled water, and with a clean glass rod, place a drop of pure aqua ammoniæ on the surface of the solution, and then a drop of the solution of nitrate of silver, an immediate precipitate will appear, the colour of which will be *yellow* if the arsenic be in large quantity, and *bluish*, if in minute portions. This test is very delicate, and in the hands of skilful experimenters will not be very liable to produce erroneous results; it is liable to the objection, that any alkaline phosphate will produce the same coloured precipitates with nitrate of silver, and hence a source of error. It is well known that this yellow preciptate is *arseniate* of silver, and that the *arseniate* of silver is red; this fact points out another mode of employing nitrate of silver to detect arsenic, which is not in use, at least I have not seen it stated in books; it is this: take a small quantity of the substance suspected to be arsenic, and pour over it a few drops of nitric acid in a watch glass, evaporate to dryness by a gentle heat; repeat this operation, red vapours will be given off during the action of the acid, and the arsenic will be converted into arsenic acid, dissolve this in a small quantity of water, and with a clean glass rod, bring first a drop of aqua ammoniæ, and then a drop of the solution of nitrate of silver, in contact with the fluid; a *red* precipitate will appear, consisting of *arseniate* of silver, if the arsenic be in considerable quantity, *reddish brown* if in a smaller quantity, and brownish *white*, if in very small quantity. A single grain of white oxyd of arsenic acidified by nitric acid, and dissolved in six ounces of water, afforded a very distinct precipitate by the above method.

If a fluid be supposed to contain arsenic, it may be evaporated to dryness and the dry matter treated with nitric acid, as above.

With these two modes of employing nitrate of silver, the one producing a *red* and the other a *yellow* precipitate there can be no difficulty, I believe, in discriminating arsenic from all other substances.

If the above observations are deemed worthy a place in your Journal, you will, by inserting them, confer an additional service on Your obedient servant,

J. F. DANA.

Cambridge, March 12, 1817.

Bill of Mortality for Rutland, (East Parish) Vermont, A. D. 1797, *to* 1816, *inclusive.* *By Ezekiel Porter.*

COMPLAINTS.	AGE.	*Male,*	*Female,*	*January,*	*February,*	*March,*	*April,*	*May,*	*June,*	*July,*	*August,*	*September,*	*October,*	*November,*	*December,*	*Total,*
Bilious Colick	47	1										1				1
Dysentery	3 62 1 8m. 1 6m. 1 7m.	5								1	2	2				5
Hemiplegia	41		1							1						1
Infant							1									1
Peripneumony Notha	70	1			1											1
Ulcer of the Liver	31		1									1				1
Unknown	8m.	1					1									1
					1		2			2	2	4				11
1798.																
Consumption	13 18	1	1								1				1	2
Empyema	15		1	1												1
Fever, Typhus	1		1								1					1
Palsy	25	1						1								1
Premature								1			1	1				3
Unknown	6m.						1									1
				1			1	2			3	1			1	9

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COMPLAINTS.	AGE.	*Male,*	*Female,*	*January,*	*February,*	*March,*	*April,*	*May,*	*June,*	*July,*	*August,*	*Septemb'r,*	*October,*	*Novemb'r,*	*December,*	*Total,*
1799.																
Consumption - - - -	42 50		2		1							1				2
Cancer - - - - - -	49		1									1				1
Convulsions - - - - -	1 1 7m.	1	1					1	1							2
Fever, Typhus - - - - -	12	1										1				1
——, Puerperal - - - - -	31		1											1		1
Fracture of the Hip Joint - - -	60		1	1												1
Infants - - - - - - - -				1			1						1	1		4
Intoxication - - - - -	7	1								1						1
Rickets - - - - - - -	1 6m.	1				1										1
Ulcer Carious - - - - - -	27	1												1		1
				2	1	1	1	1	1	1		3	1	3		15
1800.																
Consumption - - - - -	18 20	1	1				1					1				2
Dropsy - - - - - -	22 62		2	1			1									2
[*Carried forward.*				1			2					1				4

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COMPLAINTS.	AGE.	*Male*	*Female*	*January*	*February*	*March*	*April*	*May*	*June*	*July*	*August*	*Septemb'r*	*October*	*November*	*Decemb'r*	*Total*
1800.	[*Brought forward.*	1	3	1			2					1				4
Diarrhæa	3		1								1					1
Drowned	10	1				1										1
Dysentery	9m.	1								1						1
Fever, Typhus	20 35 49	2	1							3						3
——, Pulmonic	53	1			1											1
Fracture of the Cranium	32 43	2							1						1	2
Infants					1		1	1		1			1			5
Mortification	44	1			1											1
Old Age	75	1										1				1
Rickets	1 3m.		1							1						1
Unknown	1 4m. 1 9m.	1	1			1					1					2
Wound	66	1						1								1
				1	3	2	3	2	1	6	2	2	1		1	24
1801.																
Consumption	22 29		2									1		1		2
	[*Carried forward.*															

Bill of Mortality for Rutland, (East Parish) Vermont, A. D. 1797, *to* 1816, *inclusive.* *By* **Ezekiel Porter.**

COMPLAINTS.	AGE.	*Male*	*Female*	*January*	*February*	*March*	*April*	*May*	*June*	*July*	*August*	*September*	*October*	*November*	*December*	*Total*
1801.	[*Brought forward.*		2									1		1		2
Cancerous Ulcer	23	1								1						1
Casualty		1										1				1
Diarrhæa	1	1						1								1
Dropsy	31		1		1											1
Dysentery	1 6m. 1 4m.	1	1							1		1				2
Erysipelas	14		1												1	1
Fever, Typhus	26 49 41 34	3	1								2		1		1	4
Infants								1			2					3
Stone and Gravel	60	1													1	1
Tympanites Uteri	43		1	1												1
				1	1			2		2	4	3	1	1	3	18
1802.																
Consumption	16 23 37	3				1	1							1		3
Dropsy	37		1	1												1
	[*Carried forward.*			1		1	1							1		4

Bill of Mortality for Rutland, (East Parish) Vermont, A. D. 1797, *to* 1816, *inclusive.* *By Ezekiel Porter.*

COMPLAINTS.	AGE.	*Male*	*Female*	*January*	*February*	*March*	*April*	*May*	*June*	*July*	*August*	*Septemb'r*	*October*	*Novemb'r*	*December*	*Total*
1802.	[*Brought forward.*	3	1	1		1	1							1		4
Fever, Typhus	6m. 1m. 30 15 64 60 4 17 19 70 2 17 1 23 78 2	6	10		3	1	1	1			1	2	2	2	3	16
Fracture of the Cranium	40	1										1				1
Infants				1												1
				2	3	2	2	1			1	3	2	3	3	22
1803.																
Apoplexy	36	1								1						1
Bursting of a Cannon		1								1						1
Consumption	41 25 19		3			2		1								3
Diarrhæa	1 6m.		1									1				1
Dropsy	1 8m. 6m.	3														3
Dysentery	1 1 8m. 4 9 16	5				1		1	1	2						5
Fever, Bilious	49 30	1	1		1	1										2
——, Scarlet Malignant	6 11 2 15	2	2												4	4
B	[*Carried forward.*				1	4		2	1	4		1			4	20

Bill of Mortality for Rutland, (East Parish) Vermont, A. D. 1797, *to* 1816, *inclusive.* *By Ezekiel Porter.*

COMPLAINTS.	AGE.	*Male*	*Female*	*January*	*February*	*March*	*April*	*May*	*June*	*July*	*August*	*September*	*October*	*November*	*December*	*Total*
1803.	[*Brought forward.*	13	7		1	4		2	1	4		1			4	20
Small Pox - - - - -	1 1 9m.	1	1					2								2
Infants - - - - - - -																5
Palsy - - - - -	51		1										1			1
Scald - - - - - -	1 6m.							1								1
Unknown - - - -	6m. 1 1 12					1					1			1	1	4
Ulcer - - - - - - -	1 6m.	1				1										1
					1	6		5	1	4	1	1	1	1	5	34
1804. Consumption - -	20 37	1	1	1	1											2
Croup - - - - - -	4m. 16	2		2												2
Dysentery - 1 2m. 4m. 6m.	4m. 71	3	3								3	1	2			6
Fever, Typhus - - - - -	2 49	1	1			1			1							2
Old Age - - - - - - -	84	1			1											1
Ruptured Uterus - - - - -	28		1		1											1
Unknown - - - - - -	1 6m.	1				1										1
				3	3	2			1		3	1	2			15

Bill of Mortality for Rutland, (East Parish) Vermont, A. D. 1797, to 1816, inclusive. By Ezekiel Porter.

COMPLAINTS.	AGE.	*Male*	*Female*	*January*	*February*	*March*	*April*	*May*	*June*	*July*	*August*	*Septemb'r*	*October*	*November*	*Decemb'r*	*Total*
1805.																
Abscess of the Liver - - - - -	53		1										1			1
Consumption - - - - - -	57		1											1		1
Cancer - - - - - - -	68		1							1						1
Convulsions - - - - - -			1							1						1
Cholera - - - - - -	82		1	1												1
Diarrhæa - - - - - -	1		1											1		1
Dropsy - - - - - -	9m.	1				1										1
Infants - - - - - - -										2					1	3
Lumbago - - - - - -	17	1		1												1
Old Age - - - - - -	80	1								1						1
Schirrhous Liver - - - -	60	1									1					1
Worms - - - - - -	3		1	1												1
				3		1				5	1		1	2	1	14
1806.																
Consumption - - - - - -	24		1											1		1

[*Carried forward.*

Bill of Mortality for Rutland, (East Parish) Vermont, A. D. 1797, *to* 1816, *inclusive.* *By Ezekiel Porter.*

COMPLAINTS.	AGE.	*Male*	*Female*	*January*	*February*	*March*	*April*	*May*	*June*	*July*	*August*	*September*	*October*	*November*	*December*	*Total*
1806.	[*Brought forward.*		1											1		1
Convulsions	3m.		1		1											1
Fever, Typhus	15		1											1		1
——, Puerperal	34		1	1												1
Hydrocephalus Internus	2	1		1												1
Infants				1	1		1	1			1	1				6
Old Age	86	1		1												1
Worms	6		1								1					1
				4	2		1	1			2	1		2		13
1807.																
Aphtha	2		1											1		1
Consumption	60 67 24 43	1	3		1			1		1	1					4
Croup	3	1						1								1
Diabetes Mellitus	37		1					1								1
Dropsy	17		1							1						1
	[*Carried forward.*				1			3		2	1			1		8

Bill of Mortality for Rutland, (East Parish) Vermont, A. D. 1797, to 1816, inclusive. By Ezekiel Porter.

COMPLAINTS.	AGE.	Male	Female	January	February	March	April	May	June	July	August	September	October	November	December	Total
1807.	[Brought forward.	2	6		1			3		2	1			1		8
Fever, Pulmonic	61 55 48	3		2				1								3
——, Typhus	11 18	1	1						1		1					2
Infant													1			1
Mortification	20	1					1									1
Unknown	26	1													1	1
				2	1		1	4	1	2	2		1	1	1	16
1808.																
Consumption	20	1				1										1
Dropsy	68 32		2		1	1										2
Fever, Typhus	11 12 21 2 15 61	2	4							2	2	1			1	6
——, Puerperal	36 22		2			1									1	2
Infants							1	3	1							5
Old Age	76		1		1											1
Rickets	2	1					1									1
c	[Carried forward.				2	3	2	3	1	2	2	1			2	18

Bill of Mortality for Rutland, (East Parish) Vermont, A. D. 1797, to 1816, inclusive. *By Ezekiel Porter.*

COMPLAINTS.	AGE.	*Male*	*Female*	*January*	*February*	*March*	*April*	*May*	*June*	*July*	*August*	*Septemb'r*	*October*	*Novemb'r*	*December*	*Total*
1808.	[*Brought forward.*	4	9		2	3	2	3	1	2	2	1			2	18
Suicide by Arsenic	20		1		1											1
Schirrhus Uteri	44		1											1		1
					3	3	2	3	1	2	2	1		1	2	20
1809.																
Consumption	41 53	1	1		2											2
Croup	2 8 4	3							1				2			3
Fever, Typhus	36	1											1			1
——, Puerperal	31 25		2												2	2
Infants							1	1				1				3
Unknown	3		1	1												1
				1	2		1	1	1			1	3		2	12
1810.																
Consumption	25 30	1	1				1							1		2
	[*Carried forward.*															

Bill of Mortality for Rutland, (East Parish) Vermont, A. D. 1797, to 1816, inclusive. By Ezekiel Porter.

COMPLAINTS.	AGE.	*Male*	*Female*	*January*	*February*	*March*	*April*	*May*	*June*	*July*	*August*	*Septemb'r*	*October*	*November*	*Decemb'r*	*Total*
1810.	[*Brought forward.*	1	1				1							1		2
Croup	2	1						1								1
Dyspepsia	66	1							1							1
Fever, Typhus	36 4 25 57 67 52 15	6	1				1				1			4	1	7
——, Pneumonia	60		1			1										1
——, Puerperal	22		1			1										1
Hepatitis	54	1			1											1
Infants				1		1								1		3
Old Age	76		1							1						1
Palsy	62	1			1											1
Suppuration of the Brain	1	1				1										1
Unknown	4m.	1		1												1
				2	2	4	2	1	1	1	1			6	1	21
1811.																
Abortion	16		1				1									1
	[*Carried forward.*															

Bill of Mortality for Rutland, (East Parish) Vermont, A. D. 1797, to 1816, inclusive. By Ezekiel Porter.

COMPLAINTS.	AGE.	Male	Female	January	February	March	April	May	June	July	August	September	October	November	December	Total
1811.	[*Brought forward.*		1				1									1
Diarrhæa	1	1								1						1
Fever, Typhus	2 38 47 16	1	3									3			1	4
——, Rheumatic	71	1											1			1
Infant													1			1
Small Pox	2		1			1										1
Unknown	6m. 4	2					1					1				2
						1	2			1		4	2		1	11
1812.																
Consumption	57	1											1			1
Diarrhæa	3 1 1 3 1	3	2		1	2					2					5
Fever, Typhus	36 37 7 3 62 55 32 21	5	3			1					1	1	4		1	8
Infants						1		2			1				1	5
Old Age	93 78 73 68	2	2		1					1	1		1			4
					2	4		2		1	5	1	6		2	23

Bill of Mortality for Rutland, (East Parish) Vermont, A. D. 1797, to 1816, inclusive. *By Ezekiel Porter.*

COMPLAINTS.	AGE.	*Male*	*Female*	*January*	*February*	*March*	*April*	*May*	*June*	*July*	*August*	*Septemb'r*	*October*	*November*	*Decemb'r*	*Total*
1813.																
Apoplexy - - - - - - -	27	1							1							1
Burnt 5 from 1 6m. to 9 years - - -															5	5
Consumption - - - -	22 26 22	2	1		1	1	1									3
Cancer Uteri - - - - -	50		1								1					1
Drowned - - - - - - -	26	1								1						1
Diarrhæa - - - - -	9m. 2 2 74	2	2								3	1				4
Fever, Typhus - - - - -	23		1										1			1
——, and Ague - - - - -	26	1										1				1
Hydrothorax - - - - - -	62	1									1					1
Infants - - - - - - - -										1	1		1			3
Intoxication - - - - - -	55	1							1							1
Marasmus - - - - - -	4m.	1								1						1
Old Age - - - -	75 81 88	3				1					1	1				3
Prevailing Epidemic	45 56 58 52 48 2 41 3 64 2 33 29 56 21 3 1 6 22 23 73 45 1 50 42 66 50 7 50	17	11	5	4	13	3		1				1	1		28
D	[*Carried forward.*			5	5	15	4		3	3	7	3	3	1	5	54

Bill of Mortality for Rutland, (East Parish) Vermont, A. D. 1797, *to* 1816, *inclusive.* *By Ezekiel Porter.*

COMPLAINTS.	AGE.	*Male*	*Female*	*January*	*February*	*March*	*April*	*May*	*June*	*July*	*August*	*September*	*October*	*November*	*December*	*Total*
1813.	[*Brought forward.*	30	16	5	5	15	4		3	3	7	3	3	1	5	54
Sudden	27	1							1							1
Teething	18m.										1					1
Tetanus	55	1							1							1
Unknown	2	1						1								1
				5	5	15	4	1	5	3	8	3	3	1	5	58
1814.																
Consumption	22 28 39 27 34	2	3	1	1			1	1		1					5
Epilepsy	48		1							1						1
Fever, Typhus	19 36 7 17 43 20 10 28 1 63 43 2	7	5							2	3	3	2	1	1	12
——, Puerperal	29 26 24 22		4	1	1		2									4
Fracture of the Os Femoris	68		1					1								1
Infants				2	1		1		1				1			6
Murdered	27	1			1											1
	[*Carried forward.*			4	4		3	2	2	3	4	3	3	1	1	30

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COMPLAINTS.	AGE.	*Male*	*Female*	*January*	*February*	*March*	*April*	*May*	*June*	*July*	*August*	*Septemb'r*	*October*	*Novemb'r*	*December*	*Total*
1814.	[*Brought forward.*	10	14	4	4		3	2	2	3	4	3	3	1	1	30
Pneumonia	68 50	2		1	1											2
Suicide	27	1					1									1
Unknown	8m. 8	2			1		1									2
				5	6		5	2	2	3	4	3	3	1	1	35
1815.																
Consumption	26 19 19 32 54 62	2	4	1	2				1	1	1					6
Fever, Puerperal	42 30		2				2									2
Hydrothorax	61	1		1												1
Infant					1											1
Inflammation of the Bowels	7m.	1		1												1
Jaundice	14		1												1	1
Old Age	80		1									1				1
Palsy	68	1		1												1
Teething	2		1								1					1
				4	3		2		1	1	2	1			1	15

Bill of Mortality for Rutland, (East Parish) Vermont, A. D. 1797, to 1816, inclusive. By Ezekiel Porter.

COMPLAINTS.	AGE.	*Male*	*Female*	*January*	*February*	*March*	*April*	*May*	*June*	*July*	*August*	*September*	*October*	*November*	*December*	*Total*
1816.																
Consumption -	20 30 20 16 20 56 40	1	6			1	1	1	1		2			1		7
Fever, Typhus - - - - - -	17		1											1		1
——, Puerperal Convulsions - -	34		1								1					1
Infants - - - - - - - -						2										2
Old Age - - - - - -	78	1									1					1
Rickets - - - - - - -	1		1									1				1
Unknown - - - - - -	1	1										1				1
						3	1	1	1		4	2		2		14

The town of Rutland contained in 1792, 1407 persons; in 1810, 2124 persons; of which number 1226 belonged to the east parish. In 1810 the town contained 2378 persons; the east parish 1394. The town is situated in 33° 36′ north latitude, and in 2° 9′ east of Philadelphia.

The mean heat, according to Dr. Williams, 48°.6. Least 21°. Greatest 92°

Some account of an animal belonging to the genus Cervus, lately exhibited in Boston. Read before the Linnæan Society of New-England, January 11th, 1817. By George Hayward, M. D.

[Communicated for the New-England Journal of Medicine, &c.]

AN animal called by its owner, an Elk, was brought to town about a fortnight since for the purpose of exhibition. It was a male between 5 and 6 years of age, and was originally found in the Indiana Territory. As it differed in some respects from every species under the genus cervus, as described by systematic writers, it was an object of curiosity to ascertain, whether it was only a variety or really a distinct species. It was repeatedly visited while living by several members of this society, and the following observations and measurements made by Mr. Gray and myself, may be relied on for their accuracy. It had eight incisor teeth in the lower jaw, two short solitary tusks in the upper, eight grinders on each side in the lower, and eight on each side of the upper jaw. The head was one foot seven and a half inches long. Breadth across the forehead seven inches. The horns were round, branching and recurvate. There were three branches on the left horn, four on the right, no brow antlers. Greatest length of the horns three feet one and a quarter inch—greatest distance between them two feet one and a half inch, distance between the tips ten inches. Height of the body four feet four five-eight inches. Length from nose to rump six feet eleven and a half inches. Tail, white, nearly three inches long. Hair long, thick, coarse and elastic; longer about the neck; throat not carunculated. White spot of one foot diameter on the buttocks round the anus, with a distinct black border. Colour of the rest of the body, reddish gray; ends of the long hairs about the neck, almost black, which was the colour of the hair below the knees. Length of the neck from the shoulders to between the ears, two feet four inches; to between the horns, two feet seven inches. Length of the ears, nine and a half inches. There was a remarkable slit under each eye, of nearly an inch and a half in length, with a groove in the integuments leading from it to the nasal angle of the eye, of about a quarter of an inch.

In the history of quadrupeds, by Bewick, there is a description of an animal, under the name of the American Elk, which corresponds extremely well with the characters of the one exhibited here. It differs only in the circumstance of its having brow antlers. The same thing is mentioned by Dr. E. H. Smith, in an account that he published in the Medical Repository some years since, of four animals that were shewn in New-York, and supposed to be Elks. The slit under the eye, is noticed by Bewick and Dr. Smith. The latter observes, that it was supposed to communicate with the nostrils, and to be analogous to the puncta lachrymalia in the human head. It is said by some to be a provision of nature to facilitate respiration ; and others have observed, that these animals have a power of constricting the nostrils, and driving the air forcibly through this slit, which produces a loud and shrill whistle. The person who exhibited the animal in this town, stated that he had often known him to whistle through this aperture, so that he could be heard at a great distance. Notwithstanding these statements, considerable uncertainty existed as to the precise use and extent of this cavity, and I know of no author, except Cuvier, who has made any particular remarks upon the subject. In his "Anatomie Comparée," vol. 2d, p. 439th, Paris edition of 1805, he observes, that "ruminating animals have puncta lachrymalia and lachrymal tubes like man. Some genera of this order are still more remarkable for their lachrymal cavities ; (larmiers ou fosses lacrymales, in the original.) There are small excavations in the cheek, one under each eye, near the nasal angle, and communicating with this angle by a small furrow. These are found in the deer and antelope." In the 5th volume, page 251st of the same work, however, under the head of odorous excretions, he speaks of the impropriety of calling these depressions, larmiers or lachrymal cavities ; and observes, that there are many follicles in the lining membrane, that pour out a blackish, thick, unctuous fluid. He describes the size and situation of these cavities, though he assigns no use to the excretion.

A few days since, in consequence of the death of the animal, I was enabled to make a thorough examination of this slit in the integuments, as well as the excavation in the bone. Upon examining the slit under each eye, it was found, that it communicated with a cavity in the integuments, of the size of a horse chesnut, which had no other than the external opening, and was lined with a delicate polished membrane like that covering the lips, of a white colour, except a small livid spot in the centre. Corresponding with this cavity, there was, in the bone, an excavation of rather larger dimensions, owing to a considerable quantity of cellular substance that lay between the bone and integuments. This excavation was in no way connected with the nostrils, and had no foramen in any part of its bony parietes. There was but one lachrymal gland on each side, two puncta lachrymalia, one in the upper and one in the lower eyelid, which were connected with tubes that passed through bony canals, under the excavation described above, and entered the nostrils separately. There was no lachrymal sac, but the os unguis was easily found. On each side there was a parotid gland, and two glandular substances were noticed nearly in the place of the submaxillary. By opening the abdomen, it was observed, that there was four stomachs as in other ruminating animals, but no gall-bladder; the bile being carried immediately through the hepatic duct into the intestines. After a careful examination, an oil sac or vesicle was found in each of the hind legs.

Some of these particulars were noticed, on account of the diversity that is found to exist in these respects in different animals. Some, for example, have two lachrymal glands, while the elephant has neither os unguis nor lachrymal sac. Most of the animals in the class mammalia have a gall-bladder, but it is wanting in the goat, the horse, the deer, and the cetaceous tribe. The salivary glands are remarkably large in all ruminating animals, but I have not met with any account of there being two submaxillary glands on each side in any one of them. After what has been stated, it will, perhaps, be difficult to explain the use of the cavities or depressions under the eyes. ~~They may~~ possibly be intended as reservoirs for the tears, which can pass directly out of the eye, in the furrow in the integuments, into them, in cases of increased secretion, arising from any unusual excitement of the organ of vision; though I confess it ill accords with the economy of nature, to furnish a part, which can only be useful in a disordered state of some of the functions. The different accounts, that we have had, of the connexion of this slit with the nostril, and the power that animals have of forcing air through it, must of course be fabulous.

By comparing the description of the animal given above, with that of the Cervus Alces, Elk, or Moose Deer, it will be perceived at once, that they belong to distinct species. The horns of the Moose are palmate, with short beams or none; they shed them in December, while the others do not before February or March. They have a caruncle under the throat, with a tuft of hair generally growing from it. When in motion, they make a clattering noise; there is reason to believe, from an examination of the hoofs, that this is not the case with this animal. The Moose is much larger, his form is more clumsy, and his head shorter in proportion to the size of his body. The resemblance between the Cervus Elaphus, or stag, and this animal, is very considerable. They agree as to the form of their horns, their growth, the annual addition of a branch after the second year, and the time of shedding them. There is also a similarity in their habits and propensities, in the shape of the head and the general form of the body. They differ, however, in the length and colour of the hair, and very considerably in size; the stag being nearly red, with a short coat, and usually about three and a half feet high; while this animal has long hair, particularly about the under part of the neck, with black ends, and is between four and five feet in height. The female of the stag is eight months gravid, and rarely produces more than one young at a time, while this animal is said, by Dr. Smith, to be nine months gravid, and generally produces two young. It may be doubted, whether differences so important as these, can fairly be attributed to variety of climate alone. There is reason also to believe, that the animal exhibited in town, had not attained its full growth; it was considerably smaller than either of those described by Bewick and Dr. Smith, both of which were young, and would continue to grow, it was thought, for some years. With such a difference in the colour and length of the hair, in the size of the body, the period of gestation, and the number of young, it may not, perhaps, be improper to consider it another species, or, at least, a variety deserving a distinct name. As it seems to be unknown in Europe, and never to have been found, except on this continent, may it not, with propriety, be called Cervus Americanus, with the English name of American Stag?

P. S. Since writing the above, I have seen in the London Literary Panorama for January, 1817, a description of four of these animals that were then exhibited in England, and were carried from this country some months since. The English writer, considering them as non-descripts, calls them by the Indian name of Wapite. He notices the slit under the inner angle of the eye, and supposes that it is "auxiliary to the nostril," and that the animal has the power of whistling through it. He speaks of a gland situated on each side of the hind legs, under a tuft of yellowish hair, which secretes an oily fluid, with which the animal covers his coat by means of his horns, and thus renders it impervious to the rain. His account of the opening under the eye was probably obtained from the keeper, and is unquestionably erroneous.

Practical Remarks on some of the predisposing causes, and prevention, of Puerperal Fever, with cases. By WALTER CHANNING, M. D. *Lecturer on Midwifery in Harvard University.*

[Communicated for the New-England Journal of Medicine, &c.]

THE puerperal state in some seasons seems unusually liable to disease, and in such seasons puerperal fever is occasionally to be met with, it however, does not occur with the frequency, and other circumstances, which would constitute it a genuine epidemic. The cases are sporadic; and though in unusual numbers, some time elapses before a new case occurs, and practitioners are not apprehensive that new ones will happen.

If the cases that do occur under the circumstances now alluded to, were carefully investigated, some facts of an interesting and useful nature might be discovered. In some of them we might learn, that pregnancy has not advanced with merely the common occurrences of that state. We might find that its mechanical or sympathetic effects have been such, as to predispose to a state of system unfavourable to the puerperal condition. We might discover, that the patient, towards the close of pregnancy, had been exposed to great inclemency of weather, and that unusual and long continued exercise had been pursued. We might in conclusion, learn or recollect, that labour was accompanied with circumstances unusually severe; that it was protracted to an unusual length, or discovered, even before its termination, decided indications of serious disturbances in the system. Under all these views, prevention becomes a very important object; and it is the design of the following paper, to point out some of the causes which may predispose to puerperal disease, and some of the means which may be employed as preventives. The following remarks will naturally fall under the general heads of Pregnancy, and Parturition, or Labour.

Pregnancy.

During puerperal epidemics, those women have been most frequently observed to do well, who have suffered least from costiveness during pregnancy, and more especially if the bowels have been freely opened during labour. Without entering into an useless speculation, on the manner in which habitual costiveness, during pregnancy, may predispose to a condition of system unfavourable to the great and sudden changes which are among the phenomena of labour, and without endeavouring to prove, that such an habit may increase the power of other predisposing causes of such diseases, which few will deny, some means will be mentioned for obviating the habit, and preventing its consequences. Some of our best means for this end will be found in diet, medicine and exercise. With regard to diet, rules must necessarily be vague. In some individuals, when pregnant, the stomach is so irritable, as almost to reject every article of food; and to prevent the debilitating effects of almost total abstinence, the appetite and inclination must be consulted. In less severe cases, the appetite is equally capricious, and though many kinds of food might be retained, those only will be taken, which a paramount instinct seems to recommend. Should costiveness occur in such individuals, and continue with great obstinacy, diet will not be remedial, and recourse should be had to purgative medicines. The indications for their use being answered, their exhibition is to be suspended, and that their further use may not be demanded, very particular attention should be paid to diet. To rely entirely on purgatives, will be to induce occasionally a state of the bowels unfavourable to health. The natural tone of these organs may gradually be diminished, more powerful articles may be required, and by their use pregnancy itself may be disturbed, and abortion take place, or a feeble, puny child born at the full time. These effects are not mentioned as evils necessarily connected with the frequent use of purgatives during pregnancy. They may occur, however, and are therefore to be guarded against. Should costiveness be entirely neglected, or only partially obviated, the concurrence of a very few other causes, for instance, a very difficult labour, might involve serious disease in the puerperal state.

Exercise was mentioned as among the means of preventing or obviating costiveness. Exercise is an universal tonic. The vigour which it gives the organs of loco-motion is readily communicated to other organs. Under its salutary uses, the circulation is regular, without being hurried. The organs which are in immediate contact with the enlarged womb, and which must suffer some mechanical pressure, experience a temporary relief in the variety of movements of the body during exercise. Their functions, so liable to disturbance, are assisted in their labours. The bowels are among these organs, and their functions are susceptible of the influences referred to. The mind too, which in the pregnant state has so much influence over the body, is also under the beneficial uses of exercise.

Exercise considered as a predisposing cause of Puerperal disease.

Are there any circumstances in the use of exercise during pregnancy, which may render it a predisposing cause of puerperal disease? To be salutary, exercise should be neither violent nor long continued. Violent motion has always some effect on the circulating system. It has an effect on all the organs of the body. There are organs, however, which may be considered as susceptible of a wider range of motion than others, and whose contents also are decidedly influenced by sudden or violent exertions. The pregnant womb is eminently such an organ. In the early periods of pregnancy, it may be even thrown from its natural situation, and from occupying others, produce very dangerous and even fatal effects. What now have been some of the consequences of violent exercise, and sudden motion, on the pregnant and puerperal state? In the first place, abortion has been produced. The function of gestation has been destroyed, the child blighted, and prematurely expelled. In rare instances death has followed this occurrence. In one case under my care, puerperal convulsions supervened on abortion induced by great bodily exercise. In another individual, injudicious, viz. fatiguing exertions, during a removal from one house to another, were followed by sense of weight and coldness in the abdomen, a diminution of the abdominal tumour, slight uterine hæmorrhage and ultimate abortion.

Abortion, however, does not appear to be the only effect, which has followed long continued and violent exercise during pregnancy. In some cases it has appeared, when thus used, to have been among the predisposing causes of dangerous and even fatal puerperal fever. Undoubtedly the exposures necessarily incident to long journeys, if such be the manner in which exercise is taken, are also among these predisposing causes. The following facts may serve to illustrate these remarks.

Case 1. The subject of this case had endured the fatigue and exposure of a long journey about three weeks before delivery. Severe pulmonary disease was the immediate consequence. The puerperal state occurred at the close of this pulmonary affection, and before the patient had entirely retrieved the strength and health which had been necessarily prostrated by severe disease, and prompt treatment. Spurious pains of a very severe nature came on, which were relieved by prompt bleeding. True labour soon followed, and was unusually distressing and protracted. The patient was delivered of twins, both of which died soon after birth. Bleeding was deemed necessary during labour, and was practised. Genuine puerperal fever was developed on the fourth day from confinement, and though protracted to the nineteenth of the disease, it proved fatal. In this case the lungs first suffered. The uterus first discovered its morbid state by the spurious pains above mentioned. These pains were relieved by bleeding. They were very probably the precursory symptoms of that disease, which followed labour, and which ultimately proved fatal.

Case II. The patient, an unusually healthy individual, performed a long journey in the latter months of pregnancy. The journey was fatiguing, and the exposure unusually great. The individual, however, did not immediately suffer any inconvenience from either. Four days previous to the occurrence of labour, she took an airing in a coach, over a very rough road, and of some miles. Before she got home, she was seized with slight rigors; complained of a sense of weight and coldness in the abdomen, which were entirely new to her. Her head ached, and her system generally seemed suddenly to have undergone some great change. Labour occurred four days from this time. It was laborious, and unusually distressing. A very large child was delivered. It was dead, and appeared to have died some days before. The placenta came away at once. It was of a dirty yellow colour, the blood, of which there was but a very small quantity, on that part, viz. which had been attached to the uterus, was black and fetid. The placenta itself looked like a mass of washed putrid flesh. Not a drop of red blood followed its expulsion. The uterus contracted imperfectly, and on the day following delivery was found still to occupy a portion of the abdominal tumour, a fetid serous discharge took place from the uterus, the quantity was great, and the discharge perpetual; no remedies diminished it, or for a moment controlled the fever which followed. The patient died on the fourth day of the disease.

Case III, IV. In both these cases, the patients had persisted in the use of exercise. At times the exercise was violent and long continued. In both of them fatal puerperal fever followed delivery, and in both the child was dead.

If the fact be at all established, that there exists any connexion, let it be never so small, between the occurrence of abortion and puerperal fever; and the use of long continued and occasionally violent bodily motion; if they exist at all in the relation of predisposing causes, and ultimate effect, a very simple but important rule of preventive practice at once presents itself, viz. the subjects of pregnancy should carefully avoid exposing themselves to long continued and violent bodily motion.

They should be cautioned against an error by no means uncommon, that exercise is salutary in proportion to its continuance and excess. If unavoidable circumstances require great exposure, on the part of the individual, much may and will be done, to diminish its dangerous consequences, if it be known that excess cannot always be committed without hazard. The rule will be still more important, in cases where the least predisposition to any disease is observed during pregnancy, and more especially during a season, in which there is any good ground for suspicion, that any epidemic puerperal disease exists.

Disease occurring during pregnancy, considered as predisposing to Puerperal Fever.

In one of the cases already mentioned, severe pulmonary disease occurred near the close of pregnancy. In this case fatal puerperal fever followed delivery. One or two more instances will now be mentioned, in which pregnancy, in some part of it, was accompanied with severe disease.

Case V. The subject of this case had suffered severely from intermittent fever, previous to the occurrence of pregnancy. This disease had been relieved, and the patient became pregnant. In the course of pregnancy, the intermittent again discovered itself, and with symptoms as severe as before. The disease did not produce miscarriage, although its morbid effects were visible in the system. The patient was delivered at the full time. Puerperal fever soon followed on delivery, and proved fatal.

Case VI. The subject of this case was naturally very feeble. Her health had always been delicate. Former pregnancies, however, had occasioned but little inconvenience, and labour had been accomplished without unusual difficulty. During her last pregnancy, however, her health seemed sensibly impaired, and an unusual disposition to disease was discovered in the frequent formation of phlegmons on the body, which in general suppurated, and kept the patient in a state of perpetual irritation. Labour occurred at the full time. It soon became complicated with convulsions of a very violent and alarming nature. The delivery was artificially effected, and a fine healthy child born. The convulsions abated. They returned again soon after, and very soon became complicated with genuine puerperal fever. The patient died in twenty-four hours after the febrile accession.

For cases like those just mentioned, it is hardly possible to give any rules for prevention, which will admit of general application. The necessity for such rules is diminished by the fact, that severe disease occasionally occurs during pregnancy, and at times near its close, without any untoward occurrence during the puerperal state. The cases have been stated, because it is by no means unimportant to know, that fatal puerperal disease has happened during, or after labour, in individuals who have suffered severe disease during pregnancy. And farther, it can never be perfectly indifferent what treatment we make use of in a disease occurring in an unusual state of the system, as in that of pregnancy, and it may be, in a stage of it, which is soon in the order of nature to produce the most striking revolution the body can undergo.

The influence of the mind in predisposing to Puerperal disease.

Puerperal diseases occurring in lying-in hospitals, have always been severe, and very frequently fatal. This is more especially the case with regard to puerperal fever. The treatment, however, which has within a few years been found so successful in this disease in private practice, will undoubtedly be found as salutary in that of hospitals. But why is it, that in lying-in wards the cases are so numerous? One answer will be, because the individuals, subject to the disease, in such establishments are numerous, and that if there be any thing spesific or peculiar in its exciting causes, every individual is equally exposed to their action. But it may be asked, is every individual equally susceptible? In the preceding pages, some cases have been mentioned, in which it would appear, that peculiar circumstances may concur to render the puerperal state, in some measure, an unsafe one. This may be the case in some of the patients of lying-in hospitals. There still, however, remains another cause, which may tend to increase the predisposition in such patients to puerperal disease, and this is the state of the mind during pregnancy and in the puerperal state. In the establishments just alluded to, the situation of patients is peculiar. They are the witnesses of every event that happens. If the process of labour is not accomplished in the general ward, but, as in some of these establishments in Europe, is completed in another apartment, the patient is immediately returned to that she previously occupied, and lives or dies in the society of those, who either have just been confined, or are in daily expectation of this event. If we suppose the disease to be epidemic, and admit that the mind has any influence in predisposing the pregnant individual to disease, a more unfortunate arrangement can scarcely be imagined, than these charities exhibit. There are some means, however, by which this influence might be prevented, or at least diminished. In an establishment, solely devoted to lying-in patients, the apartments might be multiplied. There might, for instance, be one room for receiving patients ; one for delivery, and one for recovery. In a general hospital, however, into which the number of this order of patients is comparatively small, two wards would be quite sufficient, one for the accommodation of patients previous to confinement, the second for confinement and recovery. Should severe puerperal disease occur in a case, and labour take place in another individual, this process might be completed in the general ward, and the patient, if necessary, afterwards removed.

By an arrangement something like that now proposed, a predisposition to puerperal fever might be diminished, and as far as the mind is concerned in its production, be prevented. In private practice, during a puerperal epidemic, and at any time in which the cases of fatal disease, occurring immediately or soon after delivery, are unusually frequent, the existence of such disease, and more especially its fatal termination, should be carefully concealed from those who are soon to be confined. The energies of the mind should be preserved in their natural strength and activity, and the puerperal state, as is generally true, should be represented as perfectly safe. The only case in which the mind appeared to have much influence in predisposing to puerperal disease, came under my notice, some time before those above mentioned, and shall be very briefly stated.

Case VII. The subject of this case had been the mother of several children. Her labours had been easy, and her recoveries rapid. In her last pregnancy, her mind became gradually very much depressed. Her despondence increased, and at length life seemed hardly desirable. Labour occurred and a living healthy child was born. The revolution produced in her body, though so great, and so sudden, was not accompanied, or followed by any salutary change in the state of her mind. Without any other apparent cause, puerperal fever, very soon occurred, and within the usual time of its crisis proved fatal.

Parturition.

Having, in the preceding pages, mentioned some circumstances which, occurring during pregnancy, may predispose to puerperal disease or excite abortion, and having illustrated the remarks which were suggested, by cases, it remains as was proposed, to consider, whether the puerperal state is ever morbidly influenced by the labour, and to inquire into some of the means of prevention.

During a puerperal epidemic practitioners have observed, that all puerperal patients are equally liable to its attacks. The ease with which labour is accomplished, affords no promise of exemption from the disease. This has been very particularly adverted to by the latest and best writers on the subject. It happens however very frequently, that in the sporadic cases that happen, even though more frequent than common, we have some premonition, and some ground for suspicion in the circumstances of the cases themselves, that the situation of our patients is critical, and that severe disease, may very probably follow delivery.

This will be illustrated by the following cases :—

Case VIII. The subject of this case was thirty years of age, and this was her first confinement. The labour was exceedingly protracted, and the sufferings of the patient extreme. A very unusual rigidity of the internal organs existed, which for a long time, neither yielded to bleeding nor to such attempts as prudence sanctioned to effect dilatation. The fœtal head, when it came within reach, was found to be unusually enlarged, and this enlargement was found to be owing to water within the brain. This water was evacuated, and with very great difficulty the child was delivered. A smart rigor was experienced before delivery. Severe uterine hæmorrhage took place, which tended still farther to reduce the woman. The pulse which had been for two days previous to delivery, one hundred and thirty beats in the minute, lost none of its frequency after this event. Genuine puerperal fever occurred. The pulse increased to one hundred and fifty in a minute. The abdomen became tense and painful, the respiration short and hurried. The face continued pale through the disease. The skin hot. The mind wandered, and the patient died exhausted on the 11th day of the disease.

Case IX. In this case the labour was neither remarkable for its length nor its severity. The circulation however was so rapid, that the pulse could scarcely be counted. The mind was sensibly affected. The patient being constantly in a state bordering on delirium. After delivery the pulse remained as quick and as frequent, as before that event. The other symptoms of puerperal fever very rapidly developed themselves, and the disease was fatal on the fourth day. Examination discovered, that violent inflammation had existed in many of the abdominal viscera. The intestines adhered every where, by interposed masses of coagulating lymph, and a large serous effusion had taken place into the abdominal cavity.

Case X. This case has already been alluded to, whilst considering the possible influence of violent exercise, in predisposing to puerperal disease. This patient was nearly thirty years of age, and this was her first labour. The labour was laborious. The efforts of the uterus were strong, but not very effective. The patient being of a remarkably full habit, and some rigidity existing, blood was detracted during labour. The pulse was quick and strong. Enemata were occasionally exhibited, and during the labour, rest was for a short time procured by an anodyne draught. Some hours before delivery while the head was undoubtedly advancing, a remarkable degree of restlessness was observed in the patient. This was a new occurrence in the case. A constant and strong desire was expressed to be moved frequently from one side to the other, and an unusual impatience evinced, when any circumstance prevented an immediate compliance with the request. The patient's pulse being very strong and full, and the face much flushed, a vein was again opened, and another enema exhibited. The labour pains being very efficient, and every circumstance favorable to a speedy termination of the case, artificial means to promote delivery were thought inadmissable. The head had now arrived at the extreme parts, and apparently was on the point of being excluded, when the patient suddenly insisted on being turned to the opposite side. Though urged to be perfectly quiet, the desire was still enforced with a good deal of agitation, and the change of position was allowed. The uterine efforts were at once suspended, an universal, and violent chill came on, accompanied with peculiar mental distress. A mildly cordial draught was immediately given. The pains soon returned, and though less severe than before, the third uterine effort completed the delivery. The patient was comfortable through the day following delivery. Puerperal fever occurred the second day from confinement. One of its earliest symptoms was the restlessness already adverted to. In the progress of the disease it became absolutely uncontrollable, and of a nature to the last degree distressing. The pulse soon acquired its characteristic frequency in this complaint, and the diseased condition of the abdominal viscera was discovered by its usual symptoms. Delirium at length occurred, and the patient died on the fourth day of the disease. This case, as was said above, has been alluded to. It forms in fact the second case in this collection.

Some important circumstances have been omitted in this fuller detail, but which may be found by recurring to that case.

Case XI. The circumstances which occurred during labour, in this case, and which strongly indicated the approach, and even the actual access of disease, were great soreness and severe pain in the abdomen. These were at once noticed by the practitioner in attendance, and from careful examination were ascertained to be totally different from the usual pains that characterize labour. This case was not seen by the writer, and the state of the pulse is not known. On account of the above symptoms the patient was freely bled during labour, and the symptoms disappeared. Genuine puerperal fever was not developed until the seventh day of the puerperal state. This may have been owing to the bleeding above mentioned, and to the other evacuations incident to delivery. Prompt treatment was at once commenced, and the patient was soon convalescent.

Cases XII and XIII. In both of these cases, which happened in the same season with those above mentioned, a remarkable tendency to puerperal disease was observed during labour. In one of them, the local symptoms of childbed fever were present during that process. In both the pulse was strikingly affected. It was characterized by that extreme quickness, which is perhaps among the most striking pathognomonic symptoms of this disease. The fever that followed was severe. The pulse remarkably quick and frequent, and the abdomen both tumid, and extremely painful. The treatment in these cases produced the happiest effects. Both patients recovered.

It is unnecessary to multiply facts any farther. The writer acknowledges, however, that these are not all the cases that occurred during the seasons alluded to, viz. the autumn of 1815, and the winter and spring of 1816. Eight cases occurred in the practice of one physician. Five of these were fatal, only one of these cases, however, has been stated in this paper. But few of the cases mentioned occurred in the writer's own practice. He however, saw many of the most remarkable recorded, and is indebted to his medical brethren, for some of the facts of which he was not an eye witness. The writer would farther acknowledge, that during the time specified, cases occurred of severe and even fatal puerperal fever, in which pregnancy had been apparently exempt from accident or disease, and in which labour had been unaccompanied by any untoward circumstances, and had been accomplished with perfect ease.

What are some of the reflections suggested by the facts just related, and what are some of the means which might be adopted with a view to the prevention of the disease in question? It would appear, in the first place, that during labour, not only may puerperal fever manifest itself by its most characteristic symptoms, as has been mentioned by writers on the subject, but certain phenomena may be observed, that strongly indicate an unusual predisposition to disease. Again, it would appear, that in some cases, in which a predisposition had been produced during pregnancy, the disease may exist during labour, but be attended by symptoms so unusual, as never, for a moment, satisfactorily to indicate to us their true cause. This was remarkable in case X. Another reflection very naturally suggested is this, in all seasons, but more especially in those in which child-bed diseases are uncommonly frequent, a very special regard should be paid, to every unusual occurrence during labour. Where the patient has been known to have suffered from disease, or any other accident, during pregnancy, more particularly towards its termination, the attention should be still more alive to every unusual, or untoward occurrence. In this manner disease will be early discovered, its progress observed, and we shall be in less danger of being deceived into the opinion, that our patient is safe, because the changes immediately induced by delivery, have effected a temporary suspension of diseased phenomena: we shall, on the contrary, persist in a cautious use of those means of prevention, which may have been adopted during labour itself. These means will next be inquired into.

The means of prevention may be reduced to two. The first consists in adopting *during labour*, that treatment which experience has sanctioned as the best for puerperal fever occurring after delivery. The second, in facilitating, or rather hastening, by artificial means, the termination of the labour.

The efficacy and propriety of the first method, may be argued from the result of one of the cases in this collection, viz. the eleventh. In this case, something more than a predisposition to disease was indicated, Symptoms of puerperal fever manifested themselves. Prompt bleeding removed these symptoms, and, although the disease reappeared after delivery, this is no evidence that the first loss of blood was not highly beneficial in diminishing its violence, but leaves it probable, that had it been pushed farther, the disease would never have recurred.

It is true, that in some of the other cases mentioned, blood-letting was used. It was not, however, practised so much with a view to prevent the progress of disease, or to hinder its occurrence, as to facilitate parturition, by removing some of its obstacles. The peculiar symptoms which occurred in the cases were not removed by it; and although delivery was sooner effected, in some cases, the unusual occurrences of pregnancy did not cease at that event, but continued and became among the most prominent symptoms of the disease that followed.

That prompt blood-letting, and purging also, (which are among the best means of treating puerperal fever,) may be used with a view principally to remove threatening disease, during labour, and without danger, may be argued from the most successful treatment of puerperal convulsions. This consists in suddenly taking from the system a large quantity of blood. It might farther be proved, by recurring to cases of severe uterine hæmorrhage, in which recovery after delivery seems hardly retarded; and from the fact that where fever has happened after such hæmorrhage, the indication for bleeding has been as great as in other cases, and that when fully answered, it has been followed by the happiest result.* It is meant to be distinctly understood in this place, that let our treatment be what it may, it should be used with a view principally to remove existing symptoms, or rather that incipient or perfectly formed disease, which those symptoms manifest. Our treatment will be preventive, when unusual causes have been allowed during pregnancy, to lay the foundation of future disease, and a strong predisposition to which may be manifested during labour. It should be employed as curative, when symptoms clearly manifest that disease itself has occurred. For my own part, I have no hesitation in saying, that in such cases, the treatment which has been found most beneficial in genuine puerperal fever, is the most rational, and in by far a majority of cases will be found the most successful.

The second method proposed for prevention, consists in the use of such artificial means as have been found to facilitate and hasten delivery. These, in themselves, involve not the least hazard to the mother or child. They should be resorted to when other means fail. They should especially be employed, when the length of the labour, or any peculiar circumstances attending it, leave no doubt on the mind of the practitioner, that the general system is suffering great disturbance and injury, by the process, and which, if not remedied, will speedily be followed by dangerous consequences. If the individual, during pregnancy, have been unusually exposed to any of the predisposing causes of child-bed fever, and the labour be unusually protracted, these means should come under contemplation. If, on the contrary, no ground for such predisposition exists, but still untoward circumstances strikingly manifest themselves, we are to inquire if the proper use of these means does not involve infinitely less hazard than the indefinite continuance of ineffectual labour. Where the labour is advancing, though slowly, we are not authorized to interfere. Our interference may disturb the process and prolong it; or, by adding new sources of irritation, diminish the chances of ultimate recovery. In such a case, the means recommended under the first method, are peculiarly applicable; and by the powerful impression they are calculated to make on incipient disease, we may anticipate from their use, very favourable results. The patient, however, who, during labour, has discovered strong tendencies to disease, but which have disappeared immediately after delivery, is not hence to be presumed safe, and our preventive practice abandoned. One part of the plan, at least, should be persisted in, viz. the purgative; and Dr. Gordon, of Aberdeen, assures us, that in a very fatal puerperal epidemic in that city, of those who were freely purged the day after delivery, only one died of puerperal fever.

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* See Armstrong's highly valuable work, entitled Facts and Observations relative to the fever commonly called Puerperal.

Some authors have believed fully, that puerperal fever is a contagious disease. The writer, just mentioned, felt as convinced of this as of any fact in medicine.

Prevention, under this doctrine, consists in merely removing the patient from the society and attendance of those, who have either visited or nursed patients ill of puerperal fever. Whether the doctrine of contagion be true or false, the practice here recommended is certainly not unreasonable, and where it is possible should be adopted.

The management of the puerperal state, viz. that which immediately follows delivery, with a view principally to the prevention of puerperal fever, or other disease, may form the subject of a future communication.

Note.—Page 159, line 18, read *fatal* puerperal convulsions, &c.

Contributions to Chemistry, No. 1. By JOHN GORHAM, M. D. Professor of Chemistry in Harvard University.

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1. *Indigogene.*

THE investigation of the properties of this substance seemed to promise something interesting, from the two facts announced by Brugnatelli, that indigo, when exposed to heat, produces a vapour of a fine purplish red, by which it would appear to be assimilated to iodine, and that the substance constituting this vapour is capable of forming an amalgam with quicksilver, and is, consequently, possessed of one property peculiar to metals.*

In order to obtain indigogene, so called by Brugnatelli, from ten to fifteen grains of powdered indigo are exposed in a glass capsula to the heat of a lamp. A small quantity of water is first vaporized, after which there follows a bluish smoke of an offensive odour, the mass swells and is apparently fused, and a beautiful purplish red vapour is observed to play over its surface; the lamp is then withdrawn, indigogene condenses in crystals, and it may be easily detached from the mass beneath, by a small and thin spatula. One hundred grains of the best indigo will yield from fifteen to eighteen grains of this substance. If the indigo be of an inferior quality, the quantity obtained is not only smaller, but it is separated with more difficulty, in consequence of the softness or powdery state of the scoria. Even in its best state, it is more or less contaminated with the ashes, if the residue may be so termed, and for nice experiments requires a second sublimation.

When thus procured, it appears as an aggregate of small and delicate prismatic crystals, interlaced, of a purple red colour, and slightly iridescent. The colour of a single crystal is golden yellow. Its specific gravity is about 2.6. When exposed to the temperature of 360° Fah. it is volatilized, its vapour is dense, and it speedily crystalizes. At this degree of heat it is partly decomposed, a portion of it becoming semi-fluid, and finally concreting in the form of a black, spongy, and shining mass. The crystals of indigogene, which have undergone a second sublimation, are minute, and the mass appears comparatively dense.

It is insoluble in water.

It is dissolved by the acids. One twentieth of a grain will tinge a cubic inch of concentrated sulphuric acid of a fine green; if the proportion amount to three or four grains, the colour of the solution is so intense as to appear black. On exposing it to the air for some hours, this colour is changed to a deep blue, and the same effect is immediately produced by the addition of water. A solution of indigogene in this acid, sufficiently strong to intercept the rays of light, requires for this change one sixtieth of its weight of water.

In order to ascertain, whether the conversion of the colour from green to blue, was the consequence of the chemical agency of the water, or, in other words, whether this liquid was decomposed, and the transfer of one of its elements to the indigogene occasioned the change, the following experiments were made:—

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* Annals of Philosophy, vol. VI, p. 75.

Experiment 1. Half a cubic inch of sulphuric acid, which had just been distilled, was saturated with indigogene. Its colour was intensely green; it was introduced into a graduated tube filled with quicksilver, inverted in the mercurial apparatus, and an equal volume of distilled water, which had been recently boiled, was added. When the temperature of the mixed liquids had fallen to that of the air, the tube was examined, and a minute bubble of air was discovered at its upper part. This air had, probably, been introduced with the indigogene, or it might have been disengaged from the materials, by the high heat occasioned by their mixture. It was not more than one twentieth of an inch in diameter, and it can hardly be supposed, that it arose from the decomposition of the water.

Experiment 2. A quantity of the green coloured solution was put into a vessel containing oxygen gas, which had been previously exposed to sulphuric acid, and they were excluded from the air. After some days, no change in colour was perceptible.

Experiment 3. Sulphuric solution of indigogene was exposed, in the same way, to hydrogen, nitrogen and carbonic acid gases, but no obvious effect was produced.

The change of colour, then, from green to blue, when water is added, does not appear to depend on the decomposition of that liquid, and the transfer of one of its constituents to the indigogene, and this fact, therefore, is adverse to the opinion which has been advanced respecting the cause of the change of colour in cloths that have been dyed by indigo. The colour of the cloth, when first taken from the vat, is green, but, on exposure for some days to the atmosphere, it varies to a dark blue; and it has been supposed, that this change is the consequence of the absorption of atmospheric oxygen by the colouring matter of the cloth. But as pure oxygen, and the other gases mentioned above, when in contact with the green solution, are incapable of producing this effect, and as it is immediately rendered blue by the addition of a small quantity of water, which has been freed from air, and which does not appear to be decomposed, the most probable conclusion is, that the cloth owes its blue colour to the absorption of water, and that the effect is rather mechanical than chemical.

When this sulphuric solution is largely diluted with water, and is allowed to remain at rest some hours, it is decomposed, indigogene is precipitated, the liquid becomes light blue, and, when passed through filtering paper, colourless and transparent. This blue colouring matter is, therefore, merely mixed with the diluted acid.

In its concentrated state, the solution is decomposed by potash, soda, ammonia and lime-water, the indigogene being precipitated of a beautiful violet colour.

By adding indigogene in excess to sulphuric acid, and exposing it to heat, I endeavoured to ascertain the exact degree at which this substance is vaporized; but, although the temperature was raised to the boiling point of the acid, no vapour was disengaged. It was, probably, retained by its strong affinity for the acid. At this temperature, however, the indigogene is soon decomposed, carbon is extricated, and the colour changes to a permanent dark brown.

When nitrous acid is boiled for some time on indigogene, the latter is dissolved, and the colour of the solution is a deep yellowish red. Potash, added to it, throws down a yellowish precipitate, which is re-dissolved by adding the alkali in excess. The nitric solution contains the *bitter principle* of Fourcroy and Vauquelin, but I have not succeeded in obtaining from it the *detonating crystals*.

At a boiling heat, indigogene is dissolved by hydrochloric (muriatic) acid, forming a yellowish green solution. It is also dissolved by alcohol, sulphuric ether, and the essential oil of turpentine, the solutions in the two first liquids being light blue, in the last of a fine green colour.

It is, also, soluble in the fixed oils, but requires for this effect an high temperature, the colour varying according to the degree of heat; at first, it is light blue, then greenish blue, which changes to an amethystine colour, and, finally, to a deep and beautiful purplish-red. As the temperature is reduced, the indigogene is precipitated unaltered. Indigogene is decomposed by chlorine.

Experiment 4. Dry chlorine gas was mingled with the vapour of this substance, a dense yellowish cloud was immediately formed, the odour of hydrochloric acid became perceptible, and the receiver, in which the vapour was condensed, was found, after the experiment, to contain two or three drops of a yellow liquid, of an austere, disagreeable taste.

A watery solution of chlorine slowly acts on indigogene, and reduces its crystals to a brownish mass.

Brugnatelli, and after him Dobereiner, have stated, that indigogene is capable of amalgamating with quicksilver, and, consequently, that it must be considered as consisting of, or, at least, as containing a metallic substance. In fact it has been regarded as such; and Dobereiner has even supposed, that there may exist a number of other bodies, which may be called vegetable metals. "He heated together thirty grains of the finest Guatimalo indigo, with ten grains of mercury in a porcelain dish (constantly triturating the mixture) until the purple fumes of the indigo began to appear. He obtained a solid amalgam of mercury, which, when heated, exhaled the purple fumes of indigo. When digested in sulphuric acid, it communicated a dark blue colour. When put into nitrate of silver, crystals of silver were speedily deposited in the form of an artichoke. These being digested in sulphuric acid, coloured it blue, indicating an alloy of silver, and the metal of indigo."*

The following are some of the experiments, which I have made on this interesting subject:—

Experiment 5. A globule of mercury, weighing about ten grains, was put into a spoon of platina, with a quantity of indigogene, and exposed to the heat of a spirit lamp. When the purple fumes began to appear, the mass was constantly stirred, in order to bring the metal into contact with the vapour. The quicksilver lost, in a considerable degree, its spherical form, and had the appearance of an amalgam. When it was immersed in sulphuric acid, a deep green colour was developed, and the metal reassumed its globular form.

Experiment 6. A globule of mercury, treated as above, was well washed in repeated portions of water, in order to remove any crystals of indigogene adhering to it, was afterwards dried and thrown into the acid. No change of colour was perceptible.

Experiment 7. Quicksilver, which had been exposed to the vapour of indigogene, until its form and fluidity were impaired, and afterwards washed and dried, was put into solution of nitrate of silver. After forty-eight hours the precipitated silver was immersed in sulphuric acid, but it communicated no colour.

Experiment 8. A globule of the metal, which had been exposed to indigogene, was viewed through a microscope. Its surface was uniformly covered with a thin film of the substance, exhibiting a golden yellow colour, but no distinct crystals. When added to solution of nitrate of silver, the film collected in different parts, in distinct masses, leaving the surface of the mercury beneath highly brilliant. After twenty-four hours, the precipitated silver, taken from the surface and thrown into sulphuric acid, occasioned no change of colour, nor was any effect produced on the acid until some of the silver, which had indigogene adhering to it, was added, then the acid became green.

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* Annals of Philosophy, vol. VII, p. 37.

Experiment 9. A quantity of the supposed amalgam was put into a glass tube, bent in the shape of a retort, and connected with an inverted jar, filled with quicksilver, in the mercurial apparatus. Heat was applied to the part containing the materials; the mercury boiled, a small quantity of indigogene sublimed, but no gas passed into the receiver. The quicksilver, which had thus been vaporized, was exposed to the action of sulphuric acid, but no change of colour could be observed.

Experiment 10. Protochloride of mercury (*calomel*) was sublimed through indigogene; the upper part of the receiver contained the salt, and the lower part, the indigogene. They did not appear to have acted upon each other.

Experiment 11. Perchloride of mercury (*corrosive sublimate*) and indigogene, were intimately mixed by trituration in a mortar; the mixture was put between two watch crystals and exposed to the heat of a spirit lamp. The internal surface of the upper crystal soon became covered with a light green coloured film, a whitish vapour escaped by the sides of the glasses, but the purple fumes of the indigogene were not, at any time, during the experiment, visible. On separating the glasses, the upper one was found covered with a crystalline substance, of an uniform light green colour. This substance exhibited the following properties. Under the microscope, it appeared to be composed of crystals, in the form of flattened prisms. A portion was boiled with alcohol, which acquired a fine green colour: from this solution nitrate of silver and ammonia threw down a white, and potash a deep yellow precipitate. Some of it was gently concentrated in a watch crystal; its colour gradually heightened, and it deposited minute green coloured crystals. Water, which had been boiled on the portion left undissolved by the alcohol, and which still retained its colour, was not affected by the addition of potash or ammonia. When thrown into solution of potash, this substance remained unchanged; and they were even boiled together, without producing any obvious effect. Added to sulphuric acid, it formed a green coloured solution, which was changed to blue on the affusion of water. Exposed to heat on platina, purple fumes, succeeded by a white vapour, were produced, and the whole mass was speedily dissipated. From the phenomena, exhibited by this saline substance, it can hardly be considered as a mixture of indigogene with the perchloride. May it not rather be regarded as a triple compound of indigogene, chlorine, and metallic mercury?

Experiment 12. Indigogene and cyanide (*prussiate*) of mercury were intimately mixed; the mixture was put between two watch crystals, and heat was applied. A strong odour of hydrocyanic acid was perceived; and when the glasses were separated, minute globules of mercury were found adhering to the upper crystal, and indigogene, mixed with quicksilver, occupying the lower. The result of this experiment is interesting, as it appears to demonstrate the presence of hydrogen in indigogene, and, at the same time, to afford another proof of the nature of the salt, formerly called prussiate of mercury.

Experiment 13. A mixture of indigogene and the subsulphate of mercury was exposed to heat, in the way above mentioned; the result was the decomposition of the salt, and the separation of quicksilver in its metallic state. Some of the globules were dropped into sulphuric acid, but they had no effect in communicating colour.

Attempts were also made to ascertain, whether alloys of indigogene and some of the other metals could be obtained, and the following are some of the results.

Experiment 14. A small mass of *fusible alloy*, mixed with six or eight grains of quicksilver, was melted; indigogene was added, and they were triturated together while hot. They adhered to each other with much pertinacity. A small portion of the mass tinged sulphuric acid green; the remainder was put into a retort of the capacity of one cubic inch, connected with a mercurial apparatus, and exposed to heat. No elastic fluid passed over, but a fine crimson vapour filled the retort, and a very small quantity of a light blue powder sublimed. The compound metal, after the experiment, was white, and did not colour sulphuric acid.

Experiment 15. Indigogene was mixed with melted tin, into the substance of which it could be readily pressed, and be made to divide it into a number of distinct masses. Some of these, added to the acid, tinged it green; but the others, which were previously washed and carefully wiped, produced no such effect.

Experiment 16. Metallic arsenic, and indigogene were mixed in fine powder, and exposed, in watch crystals, to a subliming heat. During the experiment, the violet fumes were very apparent. On separating the glasses, the arsenic was found sublimed, in its usual octoedral form, and very brilliant. It neither gave purple vapours on exposure to heat, nor tinged sulphuric acid green or blue. Antimony and bismuth were also treated in the same way; but there was no evidence, that either of them existed after the experiment, in the form of alloy.

Indigogene is an inflammable substance. Its vapour takes fire by the approach of a flame, and it burns with a dense and red flame, exhaling a thick smoke. It decomposes the metallic oxyds, when exposed to heat together, and the salts, which contain a large proportion of oxygen, by percussion. Mixed with chlorate of potash, and struck forcibly on an anvil, it explodes, but not violently, and the mixture deflagrates brilliantly on exposure to a strong heat. The results of its combustion are, carbonic acid, carbonic oxyd, and aqueous vapour. Hence I conclude, that the elements of indigogene are carbon and hydrogen.

In the experiments detailed, there does not appear to me to be sufficient evidence of the truth of the opinion stated by Brugnatelli, that indigogene is capable of forming an amalgam with mercury, and, therefore, is to be considered as metallic in its nature. Even supposing the amalgam to be actually produced, this property alone is not to be regarded as decisive that the substance itself is metallic. But the appearance of the quicksilver may be explained in another way. The indigogene, I believe, is kept in contact with the metals by cohesive attraction alone; for it may be mixed with melted tin, and when the solid mass is broken, its crystals may be observed in the substance unaltered; and a globule of mercury in this state, when viewed by the microscope, seems to have merely its surface covered with a film, exhibiting the colour of indigogene, which may be removed by washing, and will leave the metal in its pure and simple state. Dobereiner supposed he had obtained a solid amalgam, by the long triture of mercury with heated indigo; but it is possible, that it appeared solid, merely in consequence of the great division of the metal by the interposed substance, as quicksilver is extinguished, as it has been termed, by oily and viscid bodies. If indigogene, as a whole, be metallic, or if it contain a metal, we must admit either, that this metal is compound, of which there is as yet no example, or that it is the basis of carbon or of hydrogen. With respect to the metallic nature of carbon, no one, but Dobereiner, I believe, has ventured to affirm it; but there is some probability, that this is the case with regard to hydrogen, at least, the phenomena which take place, when mercury is negatively electrified in contact with water, by the voltaic apparatus, appear to favour the idea. But the facts, connected with the mutual action of indigogene and the metals, do not warrant any conclusion of this kind; for the supposed amalgam, when exposed to heat, gives out purple vapours, and tinges sulphuric acid green, which colour, being the same as that which is produced in the acid by indigogene alone, is a strong argument in favour of the mechanical mixture of the two substances.

[To be continued.]

REVIEW.

First lines of the Practice of Physic. By William Cullen, m. d. *late Professor of the Practice of Physic, in the University of Edinburgh, &c. &c. With notes and observations, practical and explanatory, and a preliminary discourse, in defence of classical medicine, by* Charles Caldwell, m. d. *Two volumes 8vo. Philadelphia, Edward and Richard Parker,* 1816.

IT is somewhat singular, that while Dr. Cullen contributed, perhaps more than any other person, to diffuse among medical men those principles of physiology and of pathology which now prevail, yet his own peculiar theories scarcely find a single defender among the physicians of our day. His works, and that before us more particularly, are highly valued and are diligently read; but not for the sake of the peculiar theories they present to us. The excellence of Dr. Cullen is, that he gives his descriptions and his theories distinctly; and that he is peculiarly clear and just in his delineations of disease. As a systematic writer, he is perhaps unrivalled in this respect. In regard to individual diseases, there are no doubt many, who have given descriptions as faithful, and of course more full and minute, than those of Cullen.

But the merits of this distinguished teacher are universally known, and we have not taken up the work before us with an intention to review his writings. Our business is with *this edition* of the "first lines, &c." in which is found much matter peculiar to itself. This matter consists of a preface, and a preliminary discourse, and of numerous notes, or commentaries. In these notes, the intention of the editor is, not to explain the doctrines of the original work, but to furnish corrections in respect to theory, and to supply deficiencies in respect to practice. As the editor is a gentleman, who ranks very high among the physicians of Philadelphia, for his literary and scientific attainments, the additions which he makes to this valuable work, must be regarded with the interest, which attaches to the opinions of an individual of such a description. But this interest is much increased, by what appears in the prolegomena, viz. that this work comes out under the sanction of the present Professor of the theory and practice of physic in the University of Pennsylvania, and that he adopts it as his text book. Even more, we are informed, that the editor has had access to some of Doctor Chapman's manuscript lectures, from which he has enriched his work, and from which, in two or three instances, he has given us extracts. The opportunity of learning the doctrines and precepts inculcated in the first medical school in our country, and, if the number of its pupils be a criterion, one of the first in the world, cannot be regarded with indifference.

But it must be known, that the doctrines referred to are not those, which we have hitherto received from the Philadelphia school. Those were the doctrines of Rush; and whether correct or not, we deem it certain, that his persuasive eloquence and the force of his genius caused them to be received with very little opposition during his life by those, who resorted to his lecture room. Now that the voice of their author has ceased to resound on the ears of his pupils, these doctrines must rest on their own merits alone; and it remains to be seen, which of them are placed on stable foundations, and which upon the personal influence of the celebrated Professor. Already it seems, that under the sanction of the successor of Dr. Rush, there is a formal attempt, to overthrow his most favorite principles, and almost to expose his system to derision.

"To those, who, for the last twenty years, have been conversant with the history of medicine in the United States, it is perfectly known, that a bold and persevering attempt was made, by the late Dr. Rush, to overthrow entirely Methodical Nosology, and erect, on its ruins, his favourite hypothesis of *the Unity of Disease.* Nor was he altogether unsuccessful in the pursuit of his enterprise. By a combination of popular and imposing qualities, superadded to an ascendancy, derived from his station, as a publick teacher, he implanted in the minds of no inconsiderable portion of the physicians of America, a disbelief in the truth and value of classical medicine.

"To endeavour to counteract this evil, which, from the simplicity it appeared to have introduced into medical science, had become exceedingly seductive of indolent minds; to recall the prevalence of correct principles, touching the subdivisions and classification of disease; and, to restore to our profession the advantages of system, constitute the object of our Preliminary Discourse."—Preface to American edit. p. ix.

Accordingly, in the preliminary discourse, Dr. Caldwell points out the true basis of system and classification in science, viz. "affinity in some points, and dissimilarity in others;" and he shows the object and tendency of this classification. He illustrates its importance, and the benefits to be derived from it, by a reference to the various branches of natural history and particularly to Zoology. The only question is, whether diseases are susceptible of a similar arrangement. Dr. Rush thought that they were not, but Dr. Caldwell says that they are, since they have affinity in some points and dissimilarity in others; and subsequently he shows, that Rush himself had in effect a system of Nosology. In his endeavours to establish the differences among diseases, Dr. Caldwell is led to consider the doctrine of the unity of disease promulgated by Dr. Rush. On this doctrine were grounded the principal objections of this learned Professor to systematic nosology, and, if that can be proved to be untenable, the superstructure raised upon it must be deserted. The editor of this work contends, that the doctrine of the unity of disease implies *the unity of excitability* and *the unity of stimuli.* The editor himself seems to us, not to distinguish sufficiently between sensibility and irritability, properties essentially different and perfectly distinct; but which, by Brown and his followers, are not distinguished at all. Without, however, availing himself of this distinction, the two properties being comprehended under the term excitability, Dr. Caldwell satisfactorily disproves the doctrines in question. In truth, we doubt exceedingly whether any physician ever lived, who after one year's practice verily believed that all stimuli produce the same effects, and that all parts of the body are affected in the same mode by the same stimuli.

In the conclusions at which Dr. Caldwell arrives, in regard to the doctrine of unity of disease, and in regard to the importance of a systematic arrangement of diseases, we fully accord with him; but we would not be understood as assenting to all the opinions expressed in his preliminary discourse. In the preference which he gives to Cullen's system of nosology over any other extant, he will no doubt be supported by the majority of voices; but for ourselves, we cannot be so decided in preferring Cullen to Sauvages, not to bring into view the systems of any later writers. We shall fully agree however with Dr. Caldwell, in lamenting the imperfection of all the systems of nosology, and we think it would not be difficult to point out certain radical defects in them all. That they all have important defects, Dr. Caldwell is perfectly aware; in Cullen's system he states, that "its faults are numerous, and several of them conspicuous;" and he closes his discourse with "a brief consideration of some of the most important of them."

On the criticisms, which he makes, a few remarks must be offered on our part.

First, he objects to Cullen's definition of fever, on account of its concluding clause, *sine morbo locali primario.** It was Dr. Cullen's object by these words to point out a distinction, long recognized by good observers, between the disease he was defining, called fever, or idiopathic fever, and those diseases included by Cullen in the same class under his order Phlegmasiæ. In the phlegmasiæ, when pure, there is first a local affection, viz. an inflammation, and then the system is affected by sympathy. The affection of the system in these cases has not one uniform character; sometimes it is marked by an affection of the sanguiferous system most especially, sometimes by an affection of the chylopoietic system, sometimes by an affection of the brain, and nervous system, &c. Similar varieties appear in idiopathic fever, (if the term may be employed) but without our being able to trace them to any peculiar causes. Now Dr. Caldwell contends, that, in the cases, where we fail to discover any local disease, such a one must, nevertheless, exist; and he endeavours to prove it by showing, 1st. That it is possible for an adequate local cause to exist without exciting any sensation, and therefore if in an internal part, without being recognised. 2d. That "all febrile affections which we are capable of clearly tracing to their commencement, most certainly originate in a topical affection." 3d. That the remote causes of fever cannot "gain access to the whole system at once; they must therefore attack locally, and afterwards extend their ravages on sympathetic principles."

It is probable that our author would not believe that much was gained, should the two first arguments be admitted in all their force, unless the third was also admitted to be valid and to bear upon his point. It will suffice then to examine this third. Nor is it to be granted, at first, that all foreign causes acting on the system, operate on parts and not on the whole. The parts primarily exposed to their action are the organs of sense, the skin and the mucous membrane of the various passages having external outlets.† According to the doctrine in question, there must be some local affection on some of these parts in every case of fever, and the fever arises from sympathy of the whole system with this part.

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* The definition is Prægressis languore, lassitudinae, et aliis debilitatis signis, pyrexia, sine morbo locali primario.

† It is not necessary for the purposes of the present discussion to remark, that in cases of solution of continuity, different surfaces are exposed to the action of external causes. Nor need we embarrass the question, by bringing into view the influence of the passions, as causes of disease.

We think that Dr. Caldwell will agree, that we have represented him fairly in the foregoing statement. But if the argument be pursued, it seems to prove too much. For is any one disposed to deny that the affection of the system in pleurisy in peritoritis, &c. arises in these cases respectively from inflammation of the pleura, of the peritoneum, &c. But these are not the parts, on which the remote causes could have originally acted according to Dr. C's opinion; they are not the primary diseases; and, in accordance with the principles which he repeatedly advances, they must arise from sympathy, in consequence of disease in the skin, mucous membrane, &c. Turning, however, to those pages in the book before us, in which these opinions should have been expressed, we do not find them. The inference is, that the Editor had not arrived at such conclusions.

The truth is, that the parts first acted upon, or first touched by external causes of disease, must be those before enumerated. But, causes applied to these parts, produce disease in other and distant parts, without occasioning any evident disease in the parts first acted upon or touched; or if any thing, which can be called disease, be produced in these parts primarily, this disease is transient, and the continuance of the secondary affection is not dependent upon it. Thus, a temporary interruption of the cutaneous excretion may occasion an inflammation in the pleura. Four days afterwards, you may restore the excretion in any degree you please, the inflammation will continue in the pleura. So a temporary interruption of the catamenia, or of the lochia, will occasion inflammation in the peritoneum, or in some other part; but the restoration of the uterine discharge will not remove the disease. The disease is produced *uno ictu;* and, being produced, it goes through its stages without reference to its cause. At least, after the disease is once fairly established, this is the case.

Now we conceive, that on the same principles may be explained the occurrence of what has been called idiopathic fever; and that when it is said, that this occurs *without a primary local disease*, it is not meant to deny, that some unusual impression has been made on some point in the body. Such an unusual impression may, philosophically considered, constitute a disease; but nothing is considered a disease in the common view of the subject, of which we have not some palpable evidence. In this case, it is meant to deny, that there exists any local disease, on which the affection of the system depends for its maintenance, or continuance. It is meant to deny, that the affection of the system stands on the same ground, as that which follows common inflammation, and which may be removed by removing the inflammation. It is true, that idiopathic fever is sometimes removed at an early period, by applications made to the stomach, and sometimes by applications made to the skin. But this does not prove, that the disease depends, in these cases, on the local affections of the stomach or skin.

Believing as we do, that idiopathic fever, (febris of Cullen,) is an affection of the whole system *sui generis*, altogether distinct in its nature from the sympathetic affections produced by inflammation, we could not pass unnoticed the attempt from so respectable a quarter, to confound these different affections. But, to expose the whole ground of argument on these topics would lead us too far.

There is another subject, which is taken up in the preliminary discourse, and to which the Editor frequently refers in his notes, in which we do not perfectly accord with him. This is the subject of contagion. In condemning the loose and indistinct views, which have too often been entertained by others, he, perhaps, goes to the opposite extreme, and limits too much the evidence to be admitted in proof of contagion. The small-pox may be communicated through the atmosphere by effluvia, and, likewise, by bringing the fluid or dried virus, produced on one subject, into contact with the naked fibre of another. In examining this virus, it is not found to have any sensible or chemical properties, by which it can be distinguished from matter produced in the human subject under some other circumstances. The property of re-producing, in a fresh subject, the same disease by which it has been formed in another, is learnt only by experience. There are several other circumstances learnt by observation and experience, in regard to this disease. 1. When a person has once undergone the disease, he is incapable of undergoing it again. There are, however, some rare exceptions to this; and certain local effects may be repeatedly produced by the virus on the same subject. 2. This disease may be produced by an exceedingly small quantity of the virus, as perfectly as by a large quantity. 3. This disease may be made to occur at all seasons and in all climates. Under this head, however, it is to be remembered, that in places where it always exists, it is in certain years and seasons vastly more prevalent, than in other years and seasons. Also, it is very different in its degree of severity at different periods. 4. It occurs, at a certain fixed period, that is, about fourteen days after exposure, when produced by effluvia. After inoculation, it also affects the system at a certain period; but this is shorter than when it is communicated by effluvia. Neither of these periods is of precisely the same length in all cases, varying from two to four days, without any obvious cause. In consequence of the intervention of other diseases, they may be protracted much longer. 5. We do not know the origin of this disease; but we have reason to believe, that it has not, for several ages, been produced in any other way than by contagion.

We know, then, that small-pox is contagious; and we know, that the laws, just enumerated, exist in respect to this disease. But we do not know, that these laws are necessarily connected with its power of re-producing itself by contagion. We are not, therefore, to deny the property of contagion to any other disease, because the same laws are not found to exist in respect to such disease. If it was certain, that in regard to the mode of communication, or in regard to all or any of the laws enumerated, all other contagious diseases must resemble small-pox, the difficulty of deciding whether a disease is contagious would be very much diminished.

It certainly is possible, that a disease originating on this day, for the first time, may be contagious. It certainly is possible, that diseases frequently originating *de novo*, and in various places, may be contagious. It certainly is possible, that a disease may be contagious only among persons, who have been predisposed to it by some atmospheric influence. It is true, that a good deal of circumstantial evidence is necessary to satisfy us of the contagious character of diseases of the descriptions now given. But the possibility of their having such a character cannot be denied. On the other hand, in proportion as a disease is subject to the laws enumerated as applying to small pox, we are more ready to admit evidence in favour of its being contagious.

What, then, shall be admitted as conclusive evidence of the contagious power of a disease? We know not that one kind of evidence alone is to be admitted. Inoculation makes the evidence demonstrative; but we must not refuse to be satisfied by any evidence short of this. If an unusual disease should prevail among us, and it should be made clear, that the first person affected with the same disease had recently arrived from some other district, in which that disease had been prevalent before his departure, there would arise a strong suspicion, that this disease was contagious. Should it appear only among those, who had been, in some way, exposed to the sick, this suspicion would become more strong. Should now a considerable portion of the healthy inhabitants be removed to a neighbouring place, and should all intercourse between them and the other inhabitants be prevented, and should those who had thus removed escape from the disease, while it continued to prevail among the others, the evidence would be nearly irresistible. It is true, that even here it would be possible for us to be misled. The evidence would not be so strong as that arising from inoculation. Yet it would be such as rightfully to influence our conduct. We should in such a case take the same precautions, as if we were perfectly sure of the contagious power of the disease.

To this subject, the evidence of contagion, there are repeated references in the notes to the work before us, as well as in the preliminary discourse, and the editor seems to limit much more than we have done, the evidence which is to be deemed satisfactory.

In the preliminary discourse, there are several valuable criticisms on Cullen's nosology, which entirely correspond with our own opinions; especially those which point out inflammation as the real disease, in many disorders, arranged by Cullen under different heads. It is a regard to the external appearances only, which can in any measure justify the arrangement of such diseases under different classes and orders. It is deserting the natural for an arbitrary system, which should not be done at all, unless it is done altogether. It has not been done altogether by Cullen, nor by any other nosologist, and here is a fundamental error. That it is an error difficult to avoid is very true; but it is important to be aware of it and to search for a remedy.

In referring to the numerous and copious notes, affixed to the work under review, we must be less prolix than we have been on the preliminary discourse. We recommend the careful perusal of them all to our readers, as displaying the philosophical acuteness and the practical knowledge of the editor. It would be unnecessary for us, were we able, to attempt to give additional force to the arguments, illustrations, and precepts they contain. In all points, however, we do not fully agree with their author, and on some of those, which are most important, we shall briefly remark; and as convenience permits, we shall notice some of the passages which we are so fortunate as to approve more entirely.

Dr. Caldwell disapproves, as might be expected, the theory of fever offered by Cullen. On that point few will oppose him. But he takes occasion, while on this subject, and afterwards, on various other occasions, to reprobate the use of the term *sedative*, as applied to substances acting on the living system. We are unwilling to dispute about words; but this happens to be so convenient that we cannot give it up without a struggle. We know how much fault has been found with it heretofore, but had hoped that it was getting into better fashion, and might hereafter be employed without serious opposition. We do employ articles as medicines, the use of which is very soon followed by diminution of feeling, or of action, or of both. We have not been able, by the most diligent observation, to discover, that either an increase of feeling, or an increase of action *necessarily precedes* the effects above mentioned. They may occur, but as we have watched for them diligently and patiently in many cases and have not discovered them, we cannot admit their occurrence as certain. In the present state of our knowledge, then, we can only say, that sensation and action are diminished after the use of these articles. It is obvious that this effect is not produced by the removal of any stimulus; and if we must explain the *modus operandi* of the articles referred to, we shall say that they diminish the irritability, or the sensibility, or both, as the case may be. The reader must compare this explanation with the ingenious arguments of Dr. Caldwell on this subject, and particularly with the theory offered on page 108, vol. I.

At page 111, and following, vol. I, we find a discussion of the humoral pathology, which has recently found a very distinguished advocate in Dr. Hosack. We agree with the gentleman at Philadelphia, in "bearing deliberately our decided testimony" against this doctrine. But in the discussion of this subject, and on other accasions, Dr. Caldwell takes occasion to deny the admission of all foreign matters into the blood vessels. It is by the absorbents that all matters are conveyed into the blood vessels. Now Dr. Caldwell says "to nothing will they grant admission and conveyance, until it be digested and assimilated to an uniform mass." We acknowledge that we have been in the habit of believing that some substances unassimilated are occasionally admitted into the blood vessels; not such as do, or can produce a chemical change in the blood; nor such as become constituents of the blood after their admission. We have supposed, that these substances are simply mixed with the blood, in the same manner as we believe the fibrin, the albumen and the red globules to be merely mixed together, and not to be in chemical combination. Some of the most striking evidences in support of our opinion are found in the cases, where substances, eaten by a female, give their taste and medicinal powers to the milk. Dr. Caldwell tells us, that the substances referred to in these cases are digested and become component parts of the blood; but that in the process of excretion, there takes place a decomposition, in consequence of which they are set at liberty, and are then again in possession of their original properties.

At page 141, in the same volume the subject of quarantine is adverted to, and the wish is expressed, that the matter may be more fully inquired into, and the necessity of the practice considered impartially. It is certain, that the quarantine laws are the source of great inconvenience. If they are useless, the burden should be removed. It is in this country that the subject should be investigated; for at present, we not only suffer at home by this practice, but we give occasion, by this very circumstance, to many foreign powers to subject our trade to great embarrassment abroad, in cases where it certainly is not necessary.

At page 215, vol. I, we learn, that Dr. Caldwell adopts the opinion originally proposed by Dr. Lubbock and Mr. Allen; or rather, if we remember right, by some Italian physician first of all.—This is, that the impetus of the blood through the vessels of an inflamed part is diminished, not increased, and that the vessels are debilitated. It does not seem evident to us, that the impetus is necessarily either increased or diminished during inflammation. The enlargement of the vessels is not to be explained mechanically, but by the power of elongation possessed by the fibres of their muscular coats, which has been taught by Hunter and by Barthez. But that it is essential for vessels to be debilitated, in order to the performance of the new and extraordinary functions of inflammation—this is hard to believe. In regard to Dr. Wilson's experiments, we are satisfied that they are not worthy of any regard on this point.

While considering the process of suppuration, the editor claims for one of our countrymen, an honour, which has, we believe, been commonly thought to lie between the celebrated De Haen and John Hunter. We have never seen the dissertation by Dr. Morgan; but the British physicians will ask, whether the doctrine it asserts had not been already promulgated by Hunter, in 1765.

"Pus is not produced by a mere change in an effused and stagnant portion of fluid. It is now known to be the result of morbid glandular action. It is as genuine a secretion from the blood as bile or urine, saliva or the pancreatic juice. As already intimated, it is formed by the glandular action of the capillaries, which nature, in her wisdom and resources, enables them to assume, in order that they may relieve themselves from the superabundance of blood which oppresses them.

"For a correct theory of the formation of pus, we are indebted to an American physician—the late Dr. Morgan of Philadelphia. For the Doctor's views on this subject, see his excellent Inaugural Dissertation published in Edinburgh, in the Latin tongue, in the year 1765. For the honour of American medical literature, this essay ought to be republished, and every physician of our country should qualify himself to read it in the language in which it appears."—P. 225, vol. I.

The reference of dropsical effusions to inflammation appears to us highly correct. See p. 230. vol. I, and p. 366 and following, vol. 2. Although this doctrine is not offered as new, it appears to us not to be very generally understood. The late work of Blackall, and the paper by Dr. Wells on dropsy following scarlatina, in addition to that before us, will tend to the formation of correct opinions on this subject.

On *cynanche trachealis*, Dr. Caldwell states, that this disease is sometimes epidemic. It was so, he says, in the neighbourhood of Alexandria, in the year 1799, when Washington fell a victim to it; and in Philadelphia, in the winter of 1809—10. It is certainly rare for this disease to be epidemic, although, as a sporadic disease, we are constantly meeting it. But, we were induced to refer to this subject, partly, in order to remark that the disease which destroyed our illustrious countryman, does not seem to have been precisely *cynanche trachealis*, or the croup which is so common among children; but *cynanche laryngæa*, the disease recently described by Dr. Farre of London, and which seems to occur mostly among adults.

In respect to the pathological anatomy of croup, we find the following passage:—

"In the dissections made in this country—and they have been numerous—this preternatural membrane in the trachea and bronchiæ has rarely been found. In many, perhaps a majority of cases, but very little even of common mucus, or of any thing else that marks inflammation, has been found in the neighbourhood of the seat of the disease. We have the high authority of our very able and distinguished friend, the present professor of the theory and practice of medicine in the university of Pennsylvania, to say, that such has been the result of his experience. Such has been, in like manner, the result of our own. Yet the disease is, for the most part, if not always, of an inflammatory character, and we are bound to believe, that the membrane does occasionally exist."—P. 260, vol. I.

It has happened to us, also, to see cases, where the preternatural membrane has been wanting, although certainly not in the majority of the cases which have come within our cognizance. But, where this membrane has been wanting, mucus has been found in considerable abundance.

At p. 263, the contagious power of *cynanche parotidæa* is denied. It is hoped, that some precise observations on this subject will be published. Our own experience had been rather in favour of the common opinion, but we have not observed accurately.

Passing by many interesting subjects, we find, under the head of rheumatism, that Dr. Caldwell considers this a hereditary disease; an opinion which is supported by our own observations. In respect to the treatment of this disease, Dr. Caldwell appears to us to undervalue cathartics. In the treatment of chronic rheumatism, he claims for Dr. Chapman the honour of first using flannel rollers, a practice which has, of late, been brought forward with a good deal of formality by Dr. Balfour, of Edinburgh. By the same ingenious and learned Professor, the properties of *Juniperus Sabina*, in relieving the chronic rheumatism, have also been ascertained. On this subject, our readers will not require an apology for the following valuable, though long quotation.

"On the operation of the Juniperus Sabina, or Savin, in chronic rheumatism, we are permitted, by the kindness of Dr. Chapman, to take from his manuscript lectures, the following interesting and important extract:

"'The Savin, in its operation on the system, produces the effects of a warm, powerful, and diffusible stimulant, exciting all the secretions, with a considerable determination to the surface.

"'Baffled in my attempts to cure some of the forms of chronic rheumatism with the ordinary remedies, it is now upwards of five years, since I was led, in consequence of my speculative notions, as to the powers of this medicine, to experiment with it in this disease. During the period which has subsequently elapsed, I have prescribed it very extensively, both in public and private practice. The result of my numerous trials with it is such, that I hope it will not be deemed the language of enthusiasm, when I declare, that I hold it to be entitled to be placed at the very head of the remedies in chronic rheumatism.

"'But, for its successful application, it requires a very nice discrimination in the selection of the proper cases.

"'My enlarged experience with the medicine, has taught me some degree of certainty in its use. It is still, however, not easy, by any general description, to impart the same sort of tact, or knowledge.'

"After a description of a form of rheumatism, in which there exists great exhaustion of the system, with an unusual degree of coldness, and want of action on the surface of the body, the professor subjoins the following practical remarks :

" 'Endued with properties, such as I have alleged it to possess, it was reasonable to presume, that the Savin would prove eminently serviceable in the precise form of rheumatism, which has been here described.

" 'The primary effects of the medicine, or, at least, its sensible effects are, to heat and stimulate the whole system, producing particularly a glow on the surface, with much itching and ultimately a slight perspiration, which, however, seems to be extorted by the mere force of excitement. There are, also, sometimes, miliary eruptions.

" 'The influence of the Savin over the circulation is prodigious. The pulse, which, previously to its exhibition, is commonly small, weak, and accelerated, now becomes full, active, and comparatively slow. No portion of the system, indeed, seems to escape its wide pervading operation; every function being more or less invigorated, and especially some of the secretory offices, as the urinary, the catamenial, and, perhaps, the seminal.

" 'Contrary to a very uniform law of the animal economy, by which it seems to be ordained, that the vigour of the arterial and lymphatic apparatus should be in an inverse ratio, we have here indisputable evidence, in the speedy removal of the chalky depositions, and of the various morbid growths incident to the disease, of absorption being actively performed.

" 'After some days' continuance of the Savin, either from the mitigation of the disease, or the positive tonic power which the medicine exerts, the situation of the patient is very considerably improved, as relates to his strength, appetite, rest, and general sensations.'

"Encouraged by his successful use of Savin in one kind of chronic rheumatism, Dr. Chapman was led to administer it in another—that which is usually denominated syphilitic. Here, again, his success was highly flattering; the disease being always alleviated, if not cured, by the remedy.

"On commencing the use of the Savin, in any case, the professor's practice is, to administer to an adult from twelve to fifteen grains of the powdered leaves, three times a-day. This dose he gradually increases, until an evident effect is produced; which sometimes, as he informs us, requires 'three or four times the quantity' with which he had begun."—P. 326, vol. I.

On the subject of gout, we find many valuable remarks in the notes, and, among them, another extract from the manuscript lectures of Dr. Chapman, which we subjoin. The practice it recommends is supported, among others, by Dr. Sutton, in his late work on gout, peritonitis, &c. Dr. Sutton tried this treatment thoroughly, upon himself, and with most satisfactory results.

"It may be proper for me in this place, (says the Professor,) to state, that the treatment of podagra or regular gout, by active purging, is a very ancient practice. It indeed prevailed, with no interruption, from the earliest times, until it was prohibited by Sydenham, on purely theoretical views: it being, he observes, 'an inviolable law of nature, that the matter of the disease should be thrown out by the extremities, emetics and cathartics will have no other effect, than that of bringing back the offending matter to the bowels.'

"Enslaved by the authority of Sydenham, we have ever since, with some very limited exceptions, wholly abandoned the use of purgatives, and have been most commonly content to let the attack spontaneously exhaust itself. To envelope the limb with flannel, and to urge a patient endurance of the pain, constitute, indeed, proverbially, the amount of what is at present done in a regular paroxysm of gout.

"It appears to me, that the example of Sydenham has been, in this case, exceedingly mischievous; having led, in my opinion, to the desertion of a practice, which, if judiciously applied, is not only safe, but peculiarly calculated to overcome this most distressing disease.

"It would ill comport with my duties, were I to indulge in any minute, or lengthened disquisition respecting the nature and causes of gout. My impression, very concisely stated, is, that this disease, if not originating in, has a most intimate connexion with, certain states of the alimentary canal. I am inclined to this view of the subject, from having so frequently observed gout to commence with the symptoms which denote a disordered condition of the stomach and bowels. The precursory indications of an approaching attack of this disease are, almost invariably, flatulence, sour eructations, indigestion, depraved appetite, nausea, strong sensations of internal heat, and obstinate constipation, or a lax and disordered state of the bowels.—

—"But whether the opinion I have ventured to advance on this subject be right or wrong, (continues the Professor,) it may be confidently stated, that the practice it dictates is perfectly sound, and fully warranted by long and diversified experience.

"I have now, for several years, habitually employed purgatives in the paroxysms of gout, and with unequivocal advantage. Not content with simply opening the bowels, I completely evacuate, by active purging, the entire alimentary canal. This being accomplished, all the distressing sensations of the stomach which I have mentioned are removed, the pain and inflammation of the limb gradually subside, and the paroxysm, thus broken, speedily passes away. To effect these purposes, however, it is often necessary to recur to the remedy repeatedly.

"Though, in some instances, the operation of a single cathartic will be productive of considerable relief, it more generally requires successive purging for several days to do it.

"My practice has been, to administer every day, or every other day, a very large dose of rhubarb and magnesia, to produce a greater or lesser number of openings, according to the strength of the patient, and the violence of the case."—P. 367, vol. I.

A very valuable note on the vaccine disease, commences at page 377, partly extracted from Thomas's "Modern Practice of Physic." It closes with the following passage. This contains information which may, often, be very convenient, and which is not, we apprehend, sufficiently diffused in our country.

"From the report of the physicians of the Vaccine Pock Institution, it appears, that the matter of a single pustule, being mixed with one quarter of an ounce measure of warm water, such diluted matter excited as distinct a vaccine pock by inoculation, as an equal quantity of undiluted matter."—P. 398, vol. I.

Dr. Caldwell doubts the contagious power of the measles, and still more that of *scarlatina* and of the plague. In respect to the latter disease, we know not how to arrive at a satisfactory decision, amidst the contending evidence. We have been disposed, however, to hold the same opinion with Dr. Caldwell. But as to the two former, and especially measles, our own experience does not permit us to doubt that they are contagious.

Dr. Caldwell believes most diseases of the skin to be sympathetic, and to have their seat in the stomach. Hence, free evacuation of the alimentary canal is recommended in those diseases. Erysipelas especially, he thinks, to be very similar to gout. In respect to the treatment of erysipelas, the following passage is worthy particular notice. The practice is new to us. Has it ever been extended to gout?

"But nothing, we believe, so effectually checks the progress of erysipelatous inflammation, as the use of blisters. When the inflamed spot is so situated, and of such a size, that it can be completely covered by a blister, it rarely extends any further.

"In erysipelas of the face, we have never employed this remedy. But, for arresting the course of this disease, when affecting other parts of the body, we have repeatedly used it, with great advantage. In what our author denominates 'erysipelas phlegmonodes' of the limbs, it should never be neglected."—P. 437, vol. I.

We were somewhat surprised at page 455, to find the editor coinciding with Dr. Mead and Dr. Moseley, on the influence of the moon upon hæmorrhages. It is not intended to controvert this doctrine; but we should not have expected a recommendation of it, from so strenuous an advocate for the independence of the *solidum vivum*, as we find in the philosophical editor of this work.

The practical part of the observations in this gentleman's notes will be greatly respected; and, for this reason, we think it important to remark, that the use of *digitalis*, proposed page 488, vol. I, in *epistaxis*, seems to us very extravagant, and even unsafe. This medicine is directed "in doses of one or two grains, and to be repeated every hour or oftener, until a nausea be produced." In the first place, it is certain, that *digitalis purpurea* may often occasion very distressing symptoms, and, we suspect, even death, without the occurrence of nausea. It is, therefore, unsafe to tell the young practitioner, that nausea must be the criterion of a sufficient effect. But, it is quite unsafe to repeat this medicine so often. The characteristic and important effects of it are not manifested immediately after it is taken. These effects accumulate; at length appear in full force, somewhat suddenly too, in many cases, and will be continued in a distressing, and sometimes in an alarming degree, for several days after the use of the article has been stopped.

Among many valuable passages, we select the following, from the second volume, as exhibiting the practice said to be generally approved, in Philadelphia, in the treatment of tetanus.

"It is Dr. Physick's practice—and we cannot cite higher authority—to administer opium in injections, in doses of from five to eight grains, repeated at short intervals, until the spasms be overcome, and somewhat of a soporific effect produced; applying, in the meantime, to various parts of the body, large quantities of mercurial ointment.

"The spasms being subdued, he relaxes in the use of opium, until they manifest a disposition to return; when he immediately recurs to the same remedy, and in similar doses. In the meantime, the application of the mercurial ointment is continued, until a ptyalism be produced; when the patient is considered, in some measure, out of danger.

"Still, however, great vigilance is necessary on the part of the practitioner, lest, as is sometimes the case, the salivation should cease, and the disease return; under which circumstances, the same course must be again pursued, even, at times, with augmented vigour.

"Dr. Physick prefers giving the opium by injection, in consequence of the difficulty of swallowing, which the patient usually experiences. If requisite, he conveys aliment into the stomach by means of a tube, which he carefully introduces into that organ."—Vol. II, p. 172.

The length to which this article has already extended, forbids us to go on; else we should find a few more opinions and precepts which we should be disposed to controvert, and many to which we should give our praise. We may have seemed less liberal in commendation than in censure, in respect to the passages which we have distinctly noticed. This may be explained at once, by saying, that doubtful principles require more discussion, than those of the opposite description.

We conclude by recommending the diligent perusal of this work to all our readers. They must not imagine, that we have made them acquainted with its contents. On the contrary, our review is rather designed to be read with the work, than as a substitute for it.

Medico-Chirurgical Transactions, published by the Medical and Chirurgical Society of London. Vol. VII. Part I.—Longman and Co. 1816.

[From the London Medical and Physical Journal.]

Chemical Analysis of the Mineral Waters of Spa. By EDWIN GODDEN JONES, M. D.

WE have no doubt of the accuracy and value of this paper; but our readers will not expect us to analyse an analysis, even should they permit the use of such a pun.

History of Two Cases of Angina Pectoris. By SAMUEL BLACK, M. D. of Newry.

"At the time, (says Dr. Black,) when my first observations on the disease which has been named Angina Pectoris,* were

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"*The first of my papers is contained in the Fourth Volume of the Memoirs of the Medical Society of London, and was read to that Society, in March 1794; the second (Vol. VI.) was read in October, 1796.

communicated to the public, the affection was by no means so well understood, nor the morbid changes of structure, on which it appears to depend, so thoroughly investigated and ascertained as they have since been."

These remarks of the author may serve as our apology for passing over two cases, which however interesting, do not throw any new light on the pathology or treatment of a complaint, arising, as Mr. Hunter remarked, from a great variety of causes.

History of a very Fatal Affection of the Pudendum of Female Children. By Kinder Wood, Esq. Member of the Royal College of Surgeons, and Surgeon at Oldham.

This is a very interesting and a very important paper. The disease appears to be one of those epidemics which occur in camps, fleets, and other situations in which the human race is crowded. It assumes, as we observed when noticing Mr. Makesy's paper on Ophthalmia, all the various forms of general disease, as low fever, scurvy, or cutaneous eruptions, or of local disease, as erysipelas, sometimes in a manner (which, but for the certainty of the fact, we should hardly conceive) confined to particular parts of the body, and showing itself by ulcerated legs, ophthalmia, erysipelatous cheeks, or vesicles about the hands or about the genitals. Astruc first noticed the latter, in a fleet, denominating the disease *christallines,* and from the part on which they appeared considering them venereal. We suspect they are more common in civil life than is generally suspected.*

All these diseases are attended more or less with fever, and, if neglected, or the patient is deprived of wholesome diet and air, frequently end in sordid ulcers or gangrene. The description given by Mr. Wood is very correct in all these points. We shall transcribe only the following, as connected with an important question in medical jurisprudence. The reader will, perhaps, urge, that, in this instance, the patient was well in her general health until the local symptoms were manifest. But we must always make large allowance for the rank in life of our patient. The early or slighter complaints in the children of poor manufacturers in Manchester are very likely to be overlooked by the parents, and even by the children as soon as they are old enough to be constantly employed in earning their daily bread. No wonder, therefore, if the child's indisposition attracted no notice till the local symptoms were so violent as to make her complain.

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* See an ingenious paper by Mr. Royston in our Vol. xxiii. p. 241.

"'Jane Hampson,* aged four, was admitted an out-patient of the (Manchester) Infirmary, Feb. 11, 1791. The female organs were highly inflamed, sore and painful; and it was stated by the mother, that the child was as well as usual till the preceding day, when she complained of pain in making water. This induced the mother to examine the parts affected, when she was surprised to find the appearances above described. The child had slept two or three nights in the same bed with a boy fourteen years old; and had complained that morning of having been hurt by him in the night.

"'Leeches, and other external applications, together with appropriate internal remedies, were prescribed; but the debility increased, and on the 20th of February the child died. The The coroner's inquest was taken, previously to which the body was inspected, and the abdominal and thoracic viscera were found to have been free from disease. The circumstances above related having been proved to the satisfaction of the jury. and being corroborated by the opinion I gave, that the child's death was occasioned by external violence, a verdict of murder was returned against the boy with whom she had slept. A warrant was therefore issued against the boy, but he had absconded, a circumstance which was considered as a confirmation of his guilt, when added to the circumstantial evidence alleged against him.

"'Not many weeks however had elapsed, before similar cases occurred, in which there was no reason to suspect that external violence had been offered; and some in which it was absolutely certain, that no such injury could have taken place. A few of the patients died, though from the novelty and fatal tendency of the disease, more than common attention was paid to them. I was then convinced I had been mistaken in attributing Jane Hampson's death to external violence; and I informed the coroner of the reasons which produced this change of opinion. The testimony I gave was designedly made public, and the friends of the boy, hearing of it, prevailed upon him to surrender himself.

"'When he was called to the bar at Lancaster, the judge informed the jury that the evidence adduced was not sufficient

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"* See Medical Ethics, by Dr. Percival. Note by Mr. Ward, of Manchester, page 231.

to convict him: that it would give rise to much indelicate discussion, if they proceeded on the trial; and that he hoped, therefore, they would acquit him without calling any witnesses. With this request the jury immediately complied.'"

The following is the result of the author's own practice.

"Case I.—On January 22, 1815, I was desired to see Miss R. aged six years: she had complained three or four days of headach; had been chilly, and occasionally hot; she had been sickly, and taken little food; was dull, heavy, and languid. This morning she had complained of pain in making water: upon examination the pudendum was found inflamed; upon which I was called in.

"The inner surface of the left labium was ulcerated, as well as the clitoris; the right labium was inflamed, and the whole parts tumefied, of a dark purple hue, not unlike some kinds of erysipelas; the mons veneris was enlarged and inflamed; the perineum was inflamed and covered with aphthæ, which also encircled the anus, the discharge was thin, copious, and offensive, and had inflamed the top of the thigh, where it had been suffered to remain. The face had a peculiar paleness; the bowels were slow; the pulse quick and weak; the appetite diminished; the tongue of a dull clay colour. She was thirsty, complained of chilliness, and was indisposed for motion. The liquor plumbi acetatis dilutus was ordered as a lotion, to be applied lukewarm; and poultices made up with the same fluid were directed. A decoction of bark was also given with confectio cardiaca.

"By the use of these means, the enlargement of the parts gradually subsided, the foul bottom of the sores became red, after which the ointment of white lead was used, and the parts healed by the 14th of February, a space of seventeen days from the first attendance.

"In this case the affection again returned, but was early cured by resorting to the same remedies. The patient frequently retained the urine twenty-four hours, the pain was so violent, and obstinately resisted every inclination to empty the bowels, so that the opening remedies were obliged to be exhibited with a regular attention.

"Case II.—On the 25th of April, 1815, I saw Miss S. aged five years and a half. She had been unwell a few days previous to the 21st, when complaining of pain in voiding the urine, the parts were examined and found slightly red; they were washed with milk and water, and dusted with the lapis calaminaris. On the 22d, the inflammation had increased, and the parts were slightly excoriated. On the 23d, a thick yellow discharge was observed, the patient was getting more unwell, the bowels were slow. On the 24th, the open surfaces were enlarging, and small watery vesicles appeared upon the labia and perineum; upon the left thigh also was a large cluster; the bowels were twice opened this day by some family purgative.

"On the 25th, I saw the patient, and found both labia enlarged, and of a purple redness, with numerous small watery vesicles, upon the external surface, and also within the fissura magna. They were similar to cowpock vesicles of the third and fourth day; were found also upon the perineum, and the top of the left thigh. In some places the tops of the vesications were loosened, and showed beneath a deep foul ulcer, particularly in the cluster upon the thigh, and on the anterior part of the labia. The parts within the fissura magna were every where red and inflamed, and several small ulcers were found. The skin around the anus was painful and red; and the secretion was then copious and offensive. There was a dull headach, a quick and irritable pulse, a moist tongue, but bearing a clay coloured deposit; the motions on the 24th, were dark coloured and offensive; the patient was considerably weakened, and the face of a peculiar paleness. I advised saturnine lotions slightly warmed, and saturnine poultices without oil, to the parts, and gave small doses of pulv. rhei in a saline mixture every three hours.

"April 26.—Fresh vesications still appearing; and, when the tops of the earlier vesicles had come away, the parts beneath were deeply ulcerated. Several aphthæ were observed within the labia, upon the perineum, and around the anus.—The skin was hot and dry; the bowels open, and motions dark and offensive; with excessive pain upon voiding the urine.

"27.—The top is thrown off from the cluster of vesicles upon the thigh, as well as from the vesications upon the pudendum and perineum; the open surfaces are deep and foul, secreting largely a thin and offensive matter; the anus surrounded with aphthæ; the pulse 120; skin hot and dry; bowels open, and urine excessively hot and painful. Ordered a decoction of bark, with conf. cardiaca, recommended a little red wine to the patient, and to continue the applications to the parts.

"28.—The bowels slow; the urine has been retained thirty hours; the abdomen tender and hard; with much difficulty she was prevailed upon to void the urine, which was copious, high-coloured, and of a strong smell: the aphthæ had almost disappeared, and the diseased parts shewed a large ulcer of various depths, extending over the pudendum and perineum, down to the anus; the parts within the labia were in the same state, and a deep ulcer, but not extensive, lay upon the left thigh, on its upper and inner part; the secretion is thin, copious and offensive. The sores were ordered to be washed with the lotion as usual, and dressed with the white lead ointment. Continue the bark mixture, and increase the wine.

"29th and 30th.—The ulcerations were stationary; the same means were continued; but, as the bowels were slow, they were moved with an infusion of senna.

"May 1st.—Sores improving; the bottom becoming less foul, and discharge less offensive; pulse 90 and weak; appetite poor; sits up a little; great inclination to retain the urine; bowels open. Continue the applications and remedies.

"2d.—The sores improving, as well as the strength. This state of improvement continued regularly, till the sores were healed on the 14th. After the healing, the pudendum continued discoloured and tender, and a considerable yellow mucus discharge continued with varying quantity for the space of six or eight weeks; this was relieved by a continued use of the tinct. lyttæ, bathing the parts frequently in the day with a solution of the sulphas zinci; the shower-bath was also used with the intention of checking the secretion, as well as getting up the strength. I saw this patient on the 27th of June, when the discharge had ceased.

"Upon looking over my notes, I find that, in nine years, I have seen twelve cases; of these, I have only seen the two above related so early as to be materially serviceable; the others, being among the children of labourers, had little chance, either from the attention or punctuality of the parents, of getting over so very formidable a disease. One, a little girl of two years old, recovered, and was attacked again in the course of a fortnight, which second attack proved fatal. In a girl, five years of age, where the earlier appearances of the disease had been entirely overlooked, the mother upon finding an extensive ulcer, brought the child to me, under the idea of its having received injury from fire, which had escaped attention. The case proved fatal."

Thus, of twelve cases, ten were among the labouring poor. By what means these epidemics reach the rich, cannot always be ascertained, but probably the greater matter of surprise should be that it does not oftener occur. Among children, it is still more likely, as it is impossible to prevent nursery maids from introducing their little charges to their own relations, and even in infantile day-schools the gradations must sometimes intermix.

Cases and Observations illustrating the Influence of the Nervous System in regulating Animal Heat; by HENRY EARLE, Esq. Assistant-Surgeon to St. Bartholomew's Hospital, and Surgeon to the Foundling Hospital.

"Previous to the interesting experiments published by Mr. Brodie, in the Philosophical Transactions for 1811, it was a generally received opinion, that animal temperature depended on the chemical changes which the blood undergoes in the round of circulation. On this supposition a beautiful and apparently satisfactory theory had been constructed. This much esteemed fabric of human reasoning has, however, received a severe shock from the experiments above alluded to, which tend to establish the following facts: that, when the brain has been destroyed, animal heat ceases to be generated, notwithstanding the functions of respiration are artificially continued, and apparently all the chemical changes are produced in the lungs; and further, that an animal thus subjected to artificial respiration, cools more rapidly than one that is simply killed by decapitation, probably in consequence of the circulating blood being exposed to the cold stream of air which is introduced into the lungs. From these facts it appears, that nervous influence is essential to the production of animal heat."

Mr. Hunter has been often accused of reading little. If such was the case, ample revenge has been taken of him, as he seems not to be read at all. We have only mentioned the name of Hunter, because that name is in every surgeon's mouth—him they commemorate by an annual festival. In honour of him, a magnificent mausoleum has been erected, and lectures instituted, as if in imitation of those athletic games by which the virtues and prowess of ancient heroes were celebrated. Did then "this much esteemed fabric of human reasoning receive no severe shock" from his observations? (we avoid the word experiments for reasons which will presently appear.) Let us transcribe the following passage, published a second or third time only a year before his death.

"This power of generating heat, seems to be a property in an animal while alive. In the most perfect animals it is to preserve a standard heat; and as they are most commonly in an atmosphere colder than themselves, they have most commonly occasion to exert it; and it is therefore a power only of opposition and resistance; for it is not found to exert itself spontaneously and unprovoked; but must always be excited by the energy of some external frigorific agent, or disease; yet it is natural to such animals that this power should be called forth; as will be observed by-and-by. It does not depend *on the motion of the blood, as some have supposed,* because it likewise belongs to animals who have no circulation: and the nose of a dog, which is always nearly of the same heat in all temperatures of the air, is well supplied with blood; although we must allow, where this power is greatest, the circulation is the quickest: neither can it be said to depend upon the nervous system; for it is found in animals that have *no brain or nerves.* However, it must be allowed, that all that class who possess this power in the highest degree, have the largest brain, although this power is not in the least in proportion to the quantity of brain in that class. It is most probable that it arises from some other principle; a principle so connected with life, that it can, and does, act independently of circulation, sensation, and volition; and is that power which preserves and regulates the internal machine. This power of generating heat, is in *the highest perfection when the body is in health;* and in many deviations from that state, we find that its action is extremely uncertain and irregular; sometimes rising higher than the standard, and at other times falling much below it. Instances of this we have in different diseases, and even in the same disease, within very short intervals of time. A very remarkable one fell under my own observation, in a gentleman who was seized with an apoplectic fit; and, while he lay insensible in bed, covered with blankets, I found that his whole body would, in an instant, become extremely cold in every part, continuing so for some time; and, as suddenly, would become extremely hot. While this was going on alternately, there was no sensible alteration in his pulse for several hours.

"Being satisfied of the foregoing fact, that animals had a power of generating heat, I pursued the subject still further; not so much with a view to account for animal heat, as to observe the different phenomena, with the variations or difference in the heat in different animals."*

By the above passage it appears that Mr. Hunter proved, without the uncertainty of experiments, that animal heat does not depend "on chemical changes which the blood undergoes in the round of circulation;"—that it does not depend "on the nervous system: but on that power which preserves and regulates the machine, and which is only influenced, not generated, by external circumstances, under health and disease. Consequently, that this great philosopher made his experiments, not with a view to account for animal heat, (for this would not have been less futile than the attempt to inquire into the principle of life,) but with a view to *observe the different phenomena,* with the variations of difference in different animals.

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* Animal Economy, page 103, edit. 1792.

It is true, we are referred to an experiment [an *experimentum crucis,*] in which, after the brain was removed, respiration being kept up artificially, animal heat ceased to be generated. If, indeed, we could suppose, that the power which regulates the "internal machine remains unimpaired," when the brain is destroyed, we might then say—

> "The times have been,
> "That when the brains were out, the man would die,
> "And there an end, but now they rise again
> "With twenty mortal murders on their crowns,
> "And push us from our stools. This is more strange
> "Than such a murder is."

It certainly would have been strange, if either the generation of heat, or the secretions, had been regularly maintained, or reducible to any laws in such a condition of what is called a warm-blooded animal; or, to use Mr. Hunter's more accurate expression, in an animal capable, under common circumstances, of preserving a standard heat.

Respecting Mr. Earle's paper, we ought to remark, that it contains many judicious observations. The result of some of his experiments surprized us; but so many particulars are requisite in every experiment connected with the operations of a living animal, that we are not disposed to doubt his accuracy, though the result does not always accord with our own observations. This is only in a single instance; most of the rest confirm a law established long since by Mr. Hunter, that, though, to the feeling, inflammation seems considerably to increase the temperature of a part, yet, by experiment with the thermometer, the change never exceeds a very few degrees. It is not difficult to account for this. We are accustomed to make our remarks on inanimate matter, which, at first, raises or reduces our own temperature to its own; but, in a little while, the temperature of the two substances in contact is similar. Now, in touching a living body under fever, each part in contact maintains, as far as possible, its own standard. Hence, the increased heat of the person under fever, is restored as fast as it is communicated to a person in health, and thus gives him the sensation of a much higher temperature.

INTELLIGENCE.

Domestic.

Medical Graduates in Harvard University.

AT the semi-annual examination for the degree of Doctor of Medicine, thirteen gentlemen passed the private examination, and five the publick examination, the time of study for the rest not expiring until August. The following gentlemen read and defended dissertations at the publick examination, and afterwards received the degree of Doctor of Medicine:—

Artemas Brown, of Templeton, "On Tetanus."

J. Freeman Dana, of Cambridge, "On the Importance of Animal Chemistry."

John F. Gardner, of Lynn, "On Hydrocephalus internus."

David Osgood, of Medford, "On Hydrothorax."

George S. Sproston, of Maryland, "On wounds of the Joints."

Prevalent Diseases.

The hooping cough has been unusually prevalent and fatal in Boston, during the three last months. In a case of this disease, which was examined after death, the lungs were slightly inflamed on the outside, and uncommonly free from blood within. The mucous membrane of the trachea and bronchiæ was inflamed, and the branches of the bronchiæ full of very white mucus.

The winter epidemic, as it has been called, which has frequently threatened Boston, but never appeared in any great extent, has this season just shown itself and then vanished. A strongly marked case presented itself in the practice of Dr. Jackson. The patient was a boy, ten years old, who was violently attacked with pains in the head and limbs. He had become comatose, and a number of petechiæ appeared, before medical attendance was called for, and he died in forty-eight hours from the invasion. The pulse was, at first, 80; but, within a few hours before death, suddenly changed to 120. On examining the body, the membranes of the brain were found opaque and full of water; the ventricles contained some water. The heart was full of dark fluid blood. The lungs and abdominal viscera were not changed from their healthy state. In another case, occurring at the same time, in a subject of the same age, the boy recovered; but with a paralysis of one side, from which he is not yet well, at the end of two months. During February, numerous cases were observed, of sudden attacks of acute pain in a limb, usually shifting to the head and remaining seated there for some time. Pains in the head, especially on one side, and pain in one eye, have been remarkably common. Arising from the same epidemic cause, no doubt, has been the disposition to hydrocephalus, which has been greater than usual. Two cases of this disease may be noticed as presenting something remarkable. The subject of one was a man of about forty, uncommonly strong, active, and industrious. Of late, he had made too free a use of spiritous liquors, and occasionally displayed momentary symptoms of mental alienation, which were attributed to the operation of stimulating drink, especially as he continued to pursue his usual occupations with great ardour, till the moment he was attacked. He was suddenly seized with pain in the breast, cough and expectoration of blood; on the fourth day, he became delirious, and the cough ceased; on the sixth, he was comatose, and died. The body was examined by Dr. Warren, and presented these appearances. The stomach, liver, and other abdominal organs were in a natural state. The heart was large, and full of liquid blood. The lungs dense, inflamed. One or two considerable cysts were found, which had, probably, been formed by a collection of blood from a former occurrence of haemoptysis; a complaint to which he was occasionally subject. The mucous membrane of the lungs was in a state of high inflammation. On opening the skull, a quantity of water was discharged from the surface of the tunica arachnoidea; this membrane was opaque, and contained a quantity of water between itself and the internal covering of the brain. The surface of the brain was flattened, and exhibited a shrunken or compressed appearance. The veins, on its surface, were small. The substance of the brain was firm. When the ventricles were opened, there appeared, to the surprise of all present, a great quantity of transparent water; and, when the ventricles were emptied, they retained their form perfectly, no doubt, from having been distended for a long time. The quantity of water, collected from the brain, was about eight ounces; but as no preparation had been made to receive it carefully, much was lost. No one will believe, that these appearances of the brain were caused by a disease of five days duration. It is more probable, that the water had been collecting for a long time, in a very gradual manner. Hence the functions, dependent on the brain, were not interrupted, for they had time to accommodate themselves to the change in that organ. This case has been thought more worthy of note, since observing one very similar, lately published by the distinguished Dr. Heberden, in the London Medical Transactions, especially, as it is remarked by him, that the brain is not often enough examined, nor its appearances recorded. This case may be considered even more remarkable, than that related by Dr. Heberden; since the subject of the latter was a person eighty years old, had been occasionally unwell some time previous to death, though he possessed his intellectual faculties, until the very day on which he died. It is well remarked by Dr. Heberden, "that since a few drops of blood, a slight puncture or blow on the head, will frequently produce such derangement of the brain as to prove fatal;" we have reason to admire that wonderful power in the animal fabrick, of accommodating itself to the greatest changes, when they take place slowly, and by small degrees.

The other case, not less remarkable, is related in the words of the physician who attended.

The child of S. W. aged two years, was attacked on the third day of February, with violent symptoms of fever. After a few days, the febrile symptoms subsided, leaving the child in a state of stupor, which was now and then interrupted with violent shrieks, and appearance of much distress and agitation. The head was constantly rolling from side to side; the occiput inclined backward. The eyes were dim, pupil insensible to light, and somewhat dilated. Vision was evidently impaired or lost, as no notice was taken of objects approximated to the eye. The sense of hearing was thought to be diminished, and might be wholly lost. The left arm and leg remained immoveable; the opposite extremities were occasionally agitated. The tongue, at first, foul, afterwards became clean. The intestinal evacuations were always of a green mucus; but never occurred except from the use of purgatives; drinks were swallowed, when presented, in a wild, idiotic manner.

These symptoms resisted the use of the most powerful remedies; and about the 14th day appeared to indicate a confirmed hydrocephalus, of the most distinct character. All medicines were, therefore, omitted, and the parents resigned themselves to the loss of their child, which was daily expected. In fourteen days after, I was called to the house, and, to my great surprise, found the child still living, though insensible to external objects, yet seeming to suffer pain, as it occasionally shrieked in the most distressing manner. The pulse was slow, intermittent. The skin cold and loose from great emaciation. The mother was advised to give opiates, in order to keep the child easy, and to relieve its bowels periodically by purgatives, On a second visit, I found the pulse regular, though very slow. The eyes now and then open, and presenting a strange appearance from the enlarged state of the pupil. The hopes of the mother were revived, and I felt sufficiently encouraged to recommend to blister the head anew, and to give a purgative dose of calomel, with jalap, every other day. On the operation of these medicines, a rapid amendment took place. This child, after remaining insensible to external objects nearly four weeks, began to notice things around it; soon after to reach out to the spoon: then it attended to things calculated to amuse it; began to smile and stretch out the hand. It appears perfectly sensible now. Eats freely of solid food, and gains strength daily. The pulse continues very slow. The skin is cold. The pupil is very much dilated; but contracts, on turning the head to the light.

For a few days, there has been a slight discharge of matter from the left ear; but I cannot discover any other mark of disease in the external auditory passage.

There has not, at any time, been the slightest appearance of worms, living or dead; nor of the remains of worms. The discharges have been uniformly of a green mucus, as well during, as after the operation of the most powerful purgative, until since the other changes in the symptoms of disease. They are now quite healthy, yet appear only in consequence of taking purgatives.

Since the above was written, the intestines have begun to perform their healthy functions without medicine. The child uses its legs, can stand a little, and appears well, except that it is still feeble. The pupil of the eye remains dilated.

There cannot, in my opinion, be a question as to the propriety of considering this as an instance of recovery, from a collection of water in the brain.

Lithotomy.

An accident occurred to Dr. Warren, in performing the operation of lithotomy, which it may be useful to record. The patient was about fifty years of age; of a very gross habit, and relaxed fibre; had laboured under his complaint for many years, during the last two or three of which, he had been endeavouring to prepare his mind for the operation. His sufferings became, at length, so excruciating and so constant, that he consented to take the risk of having the stone extracted, although the state of his system, and the size of the stone, contributed to lessen the probability of success. On sounding him, the stone was readily perceived; but could not be directly struck by the instrument, which appeared rather to rub it in passing, than to strike it. On examining by the rectum, both in the upright and horizontal posture, the stone could not be perceived. The patient being properly prepared, the external incision made, and the urethra opened, the gorget was passed along the staff; but, before entering the bladder, was found to strike hard upon the stone, and its further passage was impeded. The direction of the staff was then altered; and the gorget, being pushed steadily on, and carefully kept to the staff, overcame the resistance, and a stream of urine immediately appeared. The gorget being felt to strike the stone, the staff was withdrawn; but, on passing the finger along the gorget, it appeared, that the opening into the bladder was too small to be perceived, in so deep a wound. The gorget was withdrawn, and a round sound was passed through the urethra into the bladder; then a female staff was passed through the wound till it struck the sound, where the urethra was opened, and, by means of the sound, was conducted into the bladder, and the stone was struck with it. Then a curved knife was passed along the groove of this staff into the bladder, and both, being turned to the left, a free incision was made in the bladder. The forceps was then guided by the staff to the bladder; the wound being too deep to admit of the finger's reaching the bladder. The shortest diameter of the stone was found, and then it was drawn to the wound, but was found too large to pass through it. A bistoury was, therefore, passed to the stone, and insinuated between it and the bladder, so as to cut the external angle of the wound; and then, by gradual movements, the stone was brought out, without the use of great force. On examining the stone, it was found to be large, smooth, except at one part, where, for about an inch, it was rough and covered with transparent crystals, to some of which, were attached small portions of membrane. At the largest end of the stone, was seen the mark made by the beak of the gorget; and, on examining the instrument, it appeared, that about a quarter of an inch of its edge was knocked off, where it had struck the stone. The patient did well on the three days following the operation. On the fourth, the abdomen became tense, and occasionally painful. His strength diminished rapidly, and he sunk on the seventh day. The body was examined by Dr. Warren, in company with Dr. Jackson. The swelling of the abdomen was found to arise wholly from air contained in the large intestines. A slight blush from inflammation was diffused through these intestines. The peritoneum, over the upper part of the bladder, was discoloured. The bladder had, internally, a dark coloured appearance, and, at the part corresponding with the symphysis pubis, the mark of adhesion of the stone was observed. Extending from this part to the left side of the pelvis, appeared some purulent matter, in the copious cellular membrane, which enveloped the bladder. The incision of the bladder was found to be as exact as possible, through the left half of the prostrate gland, and three quarters of an inch of the bladder. This organ was not wounded, torn, nor hurt in any other part. The rectum, on its fore part, was in perfect state. On the part attached to the coccyx, it was discoloured from a small portion of coagulated lymph. The external wound, and the scrotum, were dark coloured and gangrenous. The stone, being measured, was found to be nearly three inches in length, and five in diameter.

There are many histories of stones being inclosed in a sac of the bladder; but there was nothing of the kind here. The impossibility of discovering the stone by the rectum, the resistance opposed to the gorget, the appearance of the stone, and the appearance of the inner coat of the bladder, all concur in showing, that this stone adhered strongly to the fore part of the bladder; from which it was separated by the stroke of the gorget. The separation of the stone from the bladder, was the cause of the inflammation of the peritoneum covering it; and this inflammation, propagated by sympathy to the colon, was the cause of death; although the inflammation was not so considerable, but that the patient would have recovered, if his constitution had not been impaired by disease and confinement. The gangrenous appearance of the scrotum and external wound was not produced by contusion, as there was none applied to those parts, and the pressure, on the internal wound, was not so great, as that often employed in cases which terminate well.

The peculiar occurrence, in this case, forms an objection to the use of the gorget. The operator was led to it, in this instance, from observing, that in three preceding cases, in which he had used the knife, the cures were more slow, than where the gorget had been used; and also, from a comparison of the wound, made by the gorget and by the knife, in dead bodies, operated on and dissected for the purpose of this comparison. The wound, made by the knife, was never so exact, as to size, nor situation, as that made by the gorget. Nor can it be; since the gorget is always conducted by the staff, while the knife necessarily quits the staff to make the incision of the prostrate gland.

At the anniversary meeting of the Medical Society, of the state of New-York, at the capitol, in the city of Albany, 4th of Feb. 1817, the following physicians were elected officers:—

John Stearns, *President;* Henry Mitchell, *Vice-President;* James Low, *Secretary;* Charles D. Townsend, *Treasurer.* David Hosack, T. Romeyn Beck, James Low, Charles D. Townsend, William Pattrick, jr. *Censors.* David Hosack, John Miller, Joshua Lee, Samuel L. Mitchell, Stephen Reynolds, Joseph Gilbert, Amasa Trowbridge, *Committee of Correspondence.* Samuel L. Mitchill, T. Romeyn Beck, *Permanent Members.* James Hadly, John Murray, *Honorary Members.*

Botanical Dictionary. Mr. H. Howe, of New-Haven, has just published "A Botanical Dictionary, being a translation from the French of Louis Claude Richard, Professor of Botany, at the Medical school in Paris, with additions from Martyn, Smith, Milne, Willdenow, Acharius, &c." The translator's name is not given, but the dedication is to Professor Ives, of New-Haven.

This little work should have been called a compilation, rather than a translation, since it professes to contain, not only Richard's terms, but also those of Martyn's language of Botany, Smith's Elements, Willdenow's Principles, Milne's Botanical Dictionary, and of various modern works on the cryptogamous orders. It certainly possesses *multum in parvo;* and as it is the only Botanical Dictionary, which, to our knowledge, has been published in this country, there can be little doubt, that the compiler will find a reward for his labour, in the demand for his book, among botanical students. The language is familiar and intelligible, and peculiarly suited to obviate the difficulties of beginners.

Professor Cleaveland has published a system of mineralogy for the United States. We hope to be able to give this valuable work the notice it deserves, in some future number.

Dr. Stevens, Professor of Surgery in the Medical Institution of New York has published the second volume of his translation of Boyer's Treatise on Surgical Diseases. The translation does honour to the industry and learning of Dr. Stevens, and he merits the thanks of the profession, for giving them access to a work, which will be found to be of great use to every practitioner of Surgery.

Measurement of Medicines.—The inaccuracy which attends the common methods of administering medicines, has been noticed by some one, who makes it appear, that the tenacity of a liquid, or the size of the vessel from which it is poured will, in some instances, vary the quantity to double what was intended. So that instead of ten drops ordered, twenty shall be administered. The direction to give a tea spoonful, or a table spoonful is still more uncertain, in consequence of the great inequality of those measures. An arrangement is now making for the construction, in the Boston glasshouse, of small measures, of a determinate quantity, which may be kept by every family.

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Foreign.

Hooping Cough.—The sulphuret of potash, has been lately extolled as a remedy in this disease. The dose is six grains, repeated every four hours for an adult.

Rheumatism.—Dr. Balfour, who lately proposed the use of tight bandages for rheumatism, has now introduced an additional remedy, which he calls percussion; that is, striking the part affected briskly till the pain is removed.

Stramonium and Belladonna.—These powerful remedies, which the Germans have formerly carried through the whole circle of disease, are now getting much in fashion in England. Dr. Marcet employed the extract of stramonium, for the purpose of alleviating *severe pain*, in fourteen cases of different diseases. In four cases of sciatica, decided benefit was observed; and in two others, combined with siphylitic pains its effects were still more remarkable. In tic douloureux it was also found beneficial. Dr. Marcet commences with doses of half a grain, given three times a day, and in one case the dose was increased to six grains. The extract from the seeds was found to answer best. This was prepared by boiling a pound of the bruised seed in three gallons of water, to one gallon; and after straining the decoction, again boiling the seeds in one gallon more of water down to two quarts. Then mix the decoction's and after the whole has stood twelve hours, separate it from the fecula and oil and evaporate it in a water bath.—The oil should be separated, because it is a troublesome adjunct. A gentleman in the neighbourhood of Boston, Dr. Spring, has been much in the habit of using the leaves of stramonium, externally, for acute pains.

Belladonna has been used in combination with purgatives in *chorea* and *hooping cough*. This mode of mixing purgatives with narcotics is much extolled for the cure of spasmodic diseases.

Depletion in Fever.—The active practice of the East and West Indian physicians is making great progress in England. The use especially of blood letting in fever, is supported by a host of writers, some of them of great talents and experience. This treatment is adopted even in fevers of the typhoid character in their early stage, and apparently with much success. In a late English journal, there is published a case of *debility* cured by bleeding; and in a paper by Dr. Robertson, in order to show what quantity of blood may be lost without danger, the case of a soldier is stated, who, by an accidental rupture of the temporal artery lost eight pounds of blood *at once;* and was fit for duty in ten days after. Probably the quantity was not very accurately measured.

Observations on some causes of Deafness, with the means of removing them.

The closing of the Eustachian tube, which at last causes loss of hearing, so difficultly removed by syringing the tube, and so imperfectly remedied by piercing the tympanum, almost always arises from a catarrhal cause. Twice I have seen this closure occur so frequently at the same time, that one would have been almost justifiable in calling it epidemic. After moderating the determination of blood to the head, and the catarrhal affection, by the usual remedies, the best means of obviating the permanent closing of the tube, even after the hearing has been lost for some weeks, and the forcing of the air into the tube by expiring strongly with the mouth and nostrils shut, has been tried in vain, is to excite and maintain for several days, or even weeks, an artificial inflammation of the pharynx, attended with a great discharge of mucus. It acts as an evacuant remedy upon the neighboring mucous membrane, still turgid with the stagnant fluids produced by the catarrhal inflammation.

Two or three drachms of mezereon bark, boiled down with two ounces of honey and enough of water, to ten ounces of decoction, to which as much caustic ammonia is added as to give the mixture a biting taste upon the tongue, which requires generally from half a drachm to a drachm, is to be used as a gargle every two hours, so as to excite slight inflammation very slowly. If, therefore, upon the first trial, it should seem to act too quickly, it must be diluted with more water, and employed less frequently. In old people, still greater caution is requisite, as in them the inflammation excited is more obstinate, and is apt to be unexpectedly aggravated.

The hearing is frequently not restored, until after the inflammation has again ceased.

In the bodies of almost all old people, there is found in the innermost part of the meatus auditorius externus, a firmly attached lump of indurated earwax, which, in old age, acquires a disposition to crystallize partly in an earthly form. The earpicker only scratches off its outer surface. This lump is the cause of the dullness of hearing, which occurs slowly in healthy old people. We have as yet no very convenient method of removing it.

The tympanum of the ear is frequently destroyed in consequence of the suppuration of the external meatus auditorius; and of the small bones of the ear, the stirrup only is left, which continues to shut the fenestra ovalis. But the entrance of the cold external air into the cavity of the tympanum, stimulates the membrane lining it. Moist air swells it hygroscopically. Then undulations, which would have excited vibrations in the internal parts of the ear, even without the tympanum, or small bones, are suffocated in it, and such patients become at that time perfectly deaf. The wearing of cotton within the ear, during such weather, preserves from pain, but, on the other hand, renders the feebly observed sounds, still more obscure. At these times it is of use to wear in the meatus an artificial tympanum, consisting of a short tube of lead pressed into an elliptic form, upon the inner end of which the membrane of the swimming bladder of a small fish had been previously stretched when wet, and varnished over after it had become dry.

Taliacotian Practice.—Mr. Carpue, who succeeded so admirably in the brave Captain Latham's and another officer's cases, has produced a great sensation among the unprofessional as well as professional public. Trials are making, and more are in contemplation, to supply the loss of other parts besides the nose. To what extent the art of engraftation may be practiced, is not yet definable; but in the meantime, applications are made from *defective persons* for the supply of such parts; of course they must be *second-hand* organs, which it would be ludicrous at present to mention.

Fatal Hæmorrhage from drawing a tooth.—A man had a tooth extracted in I believe the usual circumstances; a considerable hæmorrhage ensued. The discharge was attempted to be stopped by the well known means, but in vain. The actual cautery was next employed; it succeeded for a time only; the repetition failing, the carotid artery was next tied. This measure also failed under the hands of one of the most skilful surgeons in London, from whom every thing to be accomplished by art was to be expected, but the patient died from the discharge. Was the blood in this case coagulable?

Fatal Hæmorrhage from Scarifying.—On scarifying and cupping between the shoulders in the usual way, the bleeding did not stop as was expected. The discharge continuing, the known measures were taken, but without success. It was found *that the blood did not coagulate*, and this uncoagulable state was the cause of the subject dying from the evacuation.

Inflammation of the Interior coat of vessels.—I have seen the disease seemingly of the *vasa vasorum* of the interior coat of the large blood vessels several times like inflammation described by Mr. Hodgson; but it attended other well known diseases, and I could not distinguish any peculiar symptoms referable to this inflammation like state of the blood vessels. Is it a species of erysipelas?

Eau Medicinale.—The evidence of the infusion of colchicum in wine being the renowned nostrum *eau medicinale*, is increasing, but not the evidence of its efficacy. It will, in a few years, be a most curious piece of history to account for the present evidence in its favour, when its fame shall be buried in the tomb of all the Capulets.

Mr. Samuel Young continues to practice in cancerous cases, upon the principle of pressure, in many cases with great relief, and in some with entire success; at least more benefit has been produced by his than former modes of treatment.

Edin. Med. and Surgical Journal.

END OF NO. 2, VOL. I, NEW SERIES.

The New-England Journal

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On the Hooping Cough. By WILLIAM GAMAGE, jun. M. D.

[Communicated for the New-England Journal of Medicine, &c.]

THE hooping or chin cough seems to have been less understood than any other disease of so common occurrence. It has been handed over to empiricism by almost common consent. Influenced by the aspect of a majority of its cases, physicians have been in the habit of viewing it as a mild disease, and worthy little consideration. Its mischiefs have been attributed to other causes, and when it has terminated in the death of its subject, some other disease has taken the odium. I had been accustomed to view it in this light, till the autumn of 1815, when I met with several cases, so severe in their symptoms, and at the same time, so unequivocal in their character, as to put it beyond a question, that the hooping cough, unaccompanied with any other affection, could be a formidable disease. The death of one interesting boy was so evidently the effect of this disease alone, that no room was left for deception or even doubt. A detail of his symptoms is given in case 1st.

It is but recently, that any attempt has been made to ascertain the true character and seat of this disease; at least the only source, which could lead to any certain knowledge on these subjects has been neglected; I mean the examination after death of those who have been its victims. Dr. Watt, of Glasgow, has lately obliged the profession and society with a very able work on this subject; and this appears to be the first attempt at a philosophical investigation of the disease. He relates some cases, which had been previously communicated to the public by Dr. Lettsom, and which evinced the true spirit of inquiry; but these had attracted little attention, till this notice. Dr. Watt suffered most cruelly in his own family, by the loss of two of his children by this disease. He magnanimously permitted their bodies to be examined after death. The morbid phenomena, discovered in these instances, induced him to pursue the investigation. His views as to the nature of the affection were changed; and every new case served to strengthen his conviction, that the disease belonged to the class of inflammatory complaints. He was, therefore, led to adopt a mode of practice different from those in common estimation. This consisted principally in a more liberal use of the lancet; and from this practice he experienced the happiest consequences. I shall not pretend at this time to give an account of the contents of this work. It is a most valuable acquisition to medical literature, and deserves the attention of every physician. I shall be happy, if the following histories add any thing to the information contained in this volume.

Case I.

W. J. a stout boy, two years old, had the hooping cough, which commenced sometime in the latter part of October. The cough, I was informed, became violent soon after he began to hoop. I saw him on the 10th of November. The symptoms most apparent at this time, were the severity of the fits of coughing, and some hardness of the pulse. He had a cathartic and a vomit of the tartrite of antimony, which was to be repeated occasionally. On the 27th, he had taken three effective emetics and syrup of garlic and squills in liberal quantities, but his symptoms had become progressively worse. The cathartic and the emetic repeated; nauseating doses of the tartrite of antimony and squills were frequently repeated. He was put into a warm bath, and had a bath of warm vinegar constantly applied to his chest. 19th. No amendment, but paroxysms of fever occurred twice every twenty-four hours; the breathing was hurried by slight exertion. It was particularly noticed, that when the child was awake, the breathing was short and quick, but in sleep, it seemed perfectly easy. I have remarked the same occurrence since. It seems to be a good mark, by which we may distinguish the inflammation of the mucous membrane, from pneumonia. In the latter, the breathing is at all times hurried, and performed with difficulty.

The pulse was quick and small; the cough fits severe and frequent, particularly during the night. I took from a vein on the back of the hand, about three ounces of blood, and applied a large blistering plaster to the chest.

20th. Seemed a little better; but the pulse remained unchanged. Had a cathartic of the sub. M. Hydrarg.

21st. Symptoms less favourable. The frequent and long fits of coughing; the febrile paroxysms, the hurried breathing, and the quick and small pulse, evinced the disease to be unabated. He was again bled to the amount of four ounces. The blood was quite thin, coagulated slowly, and exhibited a buffy coat. The sub. M. Hydrarg. was given, one grain every four hours.

23d. Some mitigation of the symptoms was evidently obtained by the bleeding; but they soon regained their former severity. A repetition of the bleeding was objected to. An emetic, therefore, and the warm bath were again tried.

24th. Getting worse; pulse intermits. Blister renewed upon the chest. The cough was particularly distressing during the night. Opiates of the assafoetida gave no relief.

No advantage was obtained over the symptoms. He died on the 27th. In the two last days the cough abated and he was disposed to sleep continually. The skin had been in a hot and moist state.

Dissection.

The body was examined eighteen hours after death. The lungs were of their natural colour, and appeared to be sound. They exhibited externally no mark of inflammation; had no unnatural hardness in their substance, as when death takes place from pneumonia. No tubercles, no pus could be discovered. But when cut into, a frothy and apparently semipurulent fluid issued from the air cells.

The trachea and bronchiæ were next examined. A large quantity of semipurulent mucus occupied these parts. When this was removed, the mucous membrane appeared of quite a florid colour. This was more intense in the bronchiæ, and quite conspicuous in the membranous extremities of their branches. The vessels of this membrane seemed to be injected with blood, and a bright redness of a radiated appearance was observed in different spots. The semipurulent matter was quite adhesive in the bronchiæ; the smaller branches were filled with it, and it could be pressed in abundance from the air cells. These appeared to me to be strong marks of inflammation.

The pericardium and heart were found in a natural state. The abdominal viscera were examined, but no morbid appearances discovered. The stomach had not the least mark of inflammation.

The result of this case, and the morbid phenomena, which it exhibited after death, convinced me of the propriety of bleeding in this complaint; and that in cases of so much severity no other remedy ought to be trusted. This was the first instance of a fatal termination of this affection, unaccompanied with any other that I had witnessed, and though sensible that bleeding was a proper remedy, I was not aware of the extent to which it was necessary to use it; and therefore contended less earnestly against the common prejudice, than I should have done, had I had my present conviction of its necessity. I did bleed, but not so early in the case, nor so copiously as its urgency required. I had not then seen that excellent work of Dr. Watt on this disease, but was feeling my way to truth unassisted by the light of others.

I did not witness another bad case of this complaint till January, 1817. It was frequent, especially in the autumn, and December of 1816, but it was mild and seldom required medical aid. In the latter part of January and in February, the weather became severely cold, and many bad cases of hooping cough occurred in my practice. I experienced the happiest effects from bleeding. I used it in most cases when the cough was severe, and always with certain alleviation. I have on record ten cases, whose symptoms were rendered mild by a single bleeding, when emetics and the empirical remedies in common use had given no relief. In some of these cases the fits of coughing or kinks were so violent as to cause blood to gush from the nose and ears during the paroxysms. In others there was an expectoration of blood. The subjects were from four to ten years old, and the quantity of blood taken from each was from four to eight ounces. A detail of these cases would be uninteresting; but in the following the symptoms were more complicated or more unyielding.

CASE II.

C. H. G. a delicate girl, aged seven months, had the hooping cough. It progressed mildly for about four weeks; the fits of coughing then became more severe and distressing.—

She seemed at first relieved by occasional antimonial emetics; but in a short time other bad symptoms appeared. She had febrile paroxysms; restless nights; foul bowels; water evacuated only once in twenty-four hours; pulse quick and small; breathing hurried, when awake. In sleep the breathing was perfectly easy. Bleeding seemed to be the only remedy that promised any success in this case, and this could not be effected without great difficulty. It was impossible even to feel a vein on the arm or hand, and the external jugular was so buried in fat, as to be quite indistinct. I however, succeeded in getting from this vein, about an ounce and a half of blood. This seemed to give some relief, but it was of short duration; on the second day after, I made another attempt with about the same success; but the relief seemed greater and was more lasting. All the symptoms were mitigated, except the cough, and this occurred less frequently. Her situation seemed flattering for four or five days, when after a violent kink, she had a convulsive fit. An emetic was immediately given, the bowels opened by injections, and a large blister applied to the chest. The usual medicines for moderating the cough had been given perseveringly; among these was the syrup of garlic, which appears to be a useful remedy for the cough, when there is little inflammation present. A solution of assafoetida was now added to the remedies, and the warm bath was used, but all in vain, the fits again recurred in the twenty-four hours. I attempted again to bleed, and succeeded in getting two ounces of blood from the same vein. I would have taken more, but the child could not be induced to cry or make such exertion as to cause the blood to flow. It seemed to be improved by the bleeding, and was a little roused from the stupor, in which the last fits had left it; twenty-four hours elapsed before they again recurred; but after this they did not cease, till death relieved the little sufferer.

Dissection.

The body was examined eighteen hours after death, in the presence of Dr. Hayes and Dr. Jackson. The thorax being laid open, the lungs collapsed as much as usual. They were a little more florid than in ordinary cases, particularly the middle lobe of the right lung, which had a vermilion colour. But there was no hardness or other mark of pleuritic or pneumonic inflammation. When their substance was cut into, the usual frothy fluid oozed out. The trachea was opened posteriorly, the lungs being removed from the body. The first thing noticed was an adhesive mucus of a yellowish colour in considerable quantity in the trachea and bronchiæ. This being wiped away, the mucous membrane appeared more flushed than is natural. In the lower part of the trachea, in the spaces between the cartilaginous rings, the colour was a bright red. In the bronchiæ there was a more general floridness, and this was particularly bright at the origin of their branches, and it could be traced into the membraneous parts of these tubes. No fluid of any degree of purulency was perceived in this case. The stomach and intestines had not the least mark of inflammation. The coats of the stomach were remarkably white.

The inflammation of the mucous membrane in this case was not sufficient, I conceive, to cause death. In fact the hooping cough was evidently relieved previous to the commencement of the convulsive fits; and it probably was not the cause of death, otherwise, than it might be the means of inducing the fits. It is not improbable that the brain of so tender an infant should suffer by being so frequently injected and crowded with blood, as it must necessarily have been by the violence of the coughing fits. The veins of the scalp of this infant seemed ready to burst, whenever it had a paroxysm of coughing. It is to be lamented that circumstances prevented the examination of the head, as it is not improbable that there would have been found the proximate cause of the fits and of death. Had a greater quantity of blood been obtained, previous to the commencement of the fits, I am disposed to think the result of the case might have been favourable.

Case III.

February 6th, 1817. S. B. a girl aged 3 years, had had the hooping cough in a mild way, the mother supposed, about five weeks. On the 2d, its fits of coughing were observed to be more violent than usual; they daily increased in severity. She became feverish, and breathed with difficulty; she had taken emetics and cathartics, and had the warm bath without any permanently good effect. She was asleep when I saw her, and seemed to breathe quite easy; pulse quick and rather hard, but not so small as in most similar cases; it gave to the finger a peculiar grating sensation. When the child awoke, her breathing was hurried and laborious. Her mother had noticed this difference in the breathing in her sleeping and waking hours.

The face was swollen and flushed; skin hot, and the tongue coated. Bleeding seemed to me to be the only appropriate remedy, and I proceeded to take from a vein on the back of the hand, four ounces of blood. A bath of warm vinegar was kept constantly applied to the chest.

7th. Symptoms very little improved; she had two severe febrile paroxysms during the last twenty-four hours. The bleeding was repeated, and over four ounces of blood was taken from a similar vein as yesterday. Some common medicines were given to relieve the cough.

8th. She was not so much better as to be trusted without additional bleeding. Four ounces of blood were again taken.

9th. In all repects better. The mother, an intelligent woman, remarked, that she had never witnessed such evident relief as was afforded her child by the last bleeding. She had a gentle cathartic.

10th. Symptoms not quite so favourable as yesterday, had some heat, and a pulse too quick, breathing somewhat difficult. Took away four ounces of blood.

After this there was no difficulty in the management of the case. The child entirely recovered, and is now healthy and vigorous.

The inflammation in this case appeared to me to be principally confined to the mucous membrane.

The symptoms which distinguish the inflammation of this membrane from that of common pneumonia, or pleurisy, seem to be—first, the pulse is not so small in that of the first, as in that of the latter diseases. Second, The patient has no fixed pain. Third, In pneumonia and pleurisy, the respiration, with children at least, is always short and accompanied with a sort of grunt; and it is for the most part the same, whether the patient be asleep or awake; but in the inflammation of the mucous membrane, the inspirations are deeper, and not usually attended with the grunt; and in sleep it is comparatively easy.

Case IV.

S. C. G. a healthy girl, aged three years, had the hooping cough so mildly, that she continued at school, and seemed very little incommoded by it. At the end of the fifth or sixth week she seemed more unwell, and the cough became much more violent than it had been. She complained much of her head, and was observed to hold it with both hands, when the kinks occurred. Any considerable motion, or looking steadily at an object, caused dizziness and pain in the head. She had an emetic and a cathartic; but the cough continued so severe, that blood was forced from her nose and ears during the kinks. The cathartic was repeated on two succeeding days and obtained some relief to the head; but the pulse had become very unpleasant; it was full, remarkably slow, about 65 in a minute, and intermittent. Serious apprehensions were entertained, as to what might follow. Bleeding seemed to be the proper remedy; but to do this from the jugular vein was extremely abhorrent to the parents, and it was impossible to see or feel a vein either on the arm or the back of the hand. The morning after this state of the pulse was particularly noticed, she appeared more ill, and about noon was suddenly prostrated in a state resembling a fit. I saw her immediately; she lay in a state of stupor; face flushed; skin hot; pulse very rapid and small. Bleeding could be no longer delayed; the jugular vein was opened, and between four and five ounces of blood extracted. Almost immediate relief was obtained. The present symptoms after some hours subsided; but the pulse again became slow and intermittent, but not so crowded as before. It had another peculiar trait, a strong grating sensation under the finger. Her head was relieved, but the countenance retained its look of distress and anxiety. The character of the cough had entirely changed; it was small and no longer attended with a hoop. These symptoms continued much the same for some days. She took the sub. M. Hydr. in doses of one grain every six hours; had cathartics frequently repeated, and occasionally an emetic when demanded by some peculiar symptoms. As soon as the affection of the head had subsided in a tolerable degree, the cough became more troublesome. The hoop, however, did not return, and the child slowly recovered perfect health.

I have heard from medical gentlemen, of several instances of affections of the head during the hooping cough. In one case the patient appeared to die with symptoms of hydrocephalus, and water was found in the ventricles of the brain after death. The patient of another gentleman was apprehended to have hydrocephalus, but it recovered. This patient was supposed to have been benefited by blisters alternately applied to the head and chest; and by the digitalis, which seemed to produce great evacuations of water, by the kidneys. The cessation of the kinks was observed in the last case, when the head began to be affected; and their return as the head recovered.

The remarkably slow and intermittent pulse, mentioned in the above case, was noticed in another child of the same family. He was a stout boy, about two years old; he had the complaint mildly, and except this state of the pulse, had no very severe symptom. The slowness of the pulse was pretty constantly present, but the intermission only occasionally observed.

Case V.

April 1st, 1817, I was called to S. P. a girl aged one year nearly. She was in a most distressing situation with the hooping cough. Her kinks were frequent and violent; breathing hurried and laborious; skin hot; tongue thickly coated; pulse quick and small. She had had the complaint, by account, between five and six weeks; mild at first, but, for the week past, had been getting worse. She was a fat and plump child previous to the attack of the disease; but she had become quite thin, and her skin hung loosely about her. She had been treated liberally with emetics and many empirical remedies, but had derived no apparent advantage from them. The case was of little promise; but bleeding had not been tried. I took two ounces of blood from a vein on the back of the hand, and directed her to be put into the warm bath, and after this, the vinegar bath to be kept to the chest. 2d. Symptoms a little relieved; she lost two ounces more of blood. 3d. She was in many respects better; had less fever; better nights; kinks not quite so frequent; but they were still violent and proceeded almost to strangulation; breathing also still bad. I took from the jugular vein, three ounces of blood in a full stream; she was a little faint. The orifice in the vein was covered by a piece of court plaster. Some time after, in a fit of coughing, the orifice again opened, and it was judged that about two table spoonfuls of blood were lost.

The child after this recovered rapidly, without farther medical aid.

In this case the inflammation, though principally confined to the mucous membrane, had affected in some degree, I conceive, the substance of the lungs. This is not an uncommon occurrence. Pneumonia often supervenes upon the hooping cough, and, in every such instance, it arises much more probably from the spreading of the inflammation from the mucous to the cellular membrane of the lungs, than from the influence of any external cause, to which it is generally attributed. When this takes place to any great extent, the situation of the patient is extremely hazardous, and unless promptly relieved, almost desperate. Hooping cough has terminated fatally in this way more frequently than in any other. In this state of the disease, bleeding seems to be the only remedy that merits any confidence.

The following cases are examples of the disease in this form.

CASE VI.

February 14th, Abraham A. a lad aged six years, had the hooping cough mildly about five weeks, when the kinks became severe, and continuing so about a week, he was suddenly attacked, six hours before I saw him, which was in the evening, with difficulty of breathing, and an obtuse pain in the side. Skin hot; pulse rapid and small. Took from him immediately six ounces of blood, and applied a bath of warm vinegar to his chest.

15th. Morning; symptoms very little improved. Bled him to the amount of four ounces, and directed a large blister to the chest.

16th. Heat and pain diminished; breathing performed with more ease; but the pulse still quick and small. Four ounces more of blood were taken.

After this he was convalescent. The pneumonic symptoms disappeared, and with them the hooping cough. He had a slight cough, but it soon left him.

CASE VII.

A. A. Creech, eight years old, began to be affected with the hooping cough, about the middle of January. It progressed mildly for the first weeks. He occasionally expectorated a little blood, and bled at the nose; but had no other unpleasant symptom, except, almost from the first, a quick and sharp pulse. In the sixth week, the fits of coughing became more severe. February 22d, I saw him in the morning in a strong paroxysm, and his pulse was worse than it had been. It was thought prudent to bleed him, and four ounces of blood were extracted from the arm. It was quite florid, and coagulated slowly. I was called to him again in the evening. He had some pain in the chest, laborious breathing, hot skin, and a pulse as rapid as could be counted. These symptoms, with-

-out any exposure, commenced about seven hours after the bleeding in the morning. A repetition of the bleeding seemed to me the only chance of relief; six ounces of blood were, therefore, immediately taken from him. It coagulated sooner than in the morning, and had a firm buff. A large blister was applied to the chest.

23d. Breathing easier; pain less; skin cool; but the pulse very little improved. Bled him to the amount of four ounces.

The pulse remained quick and small, but other bad symptoms disappeared. Not a vestige of the hooping cough remained. A slight cough, which he had, gradually subsided. His appetite returned, and he was able to be up and play with his mates. But he made no progress. Pulse did not improve; and, after a few days, he evidently grew worse. His flesh diminished; food rejected; and he had frequent febrile flushes. But he had no pain, slept well, and breathed with perfect ease. His situation, however, was bad. An emetic was administered; small doses of the sub-muriat. hydr. often repeated, and a vesicatory applied over the place of the former pain. He obtained little advantage from these means, but continued in much the same state, till March 18th, when he was suddenly taken with strangulation, and after a minute, coughed, and brought up, according to the report of his attendant, a roundish substance, of the bigness of a large walnut, consisting of a soft substance, which she said looked like matter, and of some small round bodies, which were harder. This was followed by considerable fresh blood. In about twenty-four hours after, a similar substance was coughed up, of less bulk, and accompanied with less blood. They were most unfortunately not preserved for my inspection. From the description, and from what I have seen in some dissections, I am disposed to think, that they were clusters of small tubercles, which had been insulated by the suppuration of the surrounding substance, and, being separated, made their way into a neighbouring bronchial tube by ulceration, and were thence ejected. Immediately after this occurrence the lad began to mend; appetite good, and the food retained. He has since become stout and active.

CASE VIII.

S. H. aged twenty months, had the hooping cough in January, 1817. When the disease had continued about four weeks, she began to suffer from the violence of the cough, and frequent febrile paroxysms. She was bled two days in succession, to the amount of two ounces each time. The symptoms were relieved, but not removed. Fifteen days after this, I was again called to her. She was very ill with pneumonia, and had been so for two or three days. Three ounces of blood were taken from her. I was not permitted to repeat the operation. Blistering, emetics, &c. were tried in vain; she died in about seven days from the attack.

The body was examined twenty-four hours after death. The lungs were generally of a dark colour, and crowded with blood. The right lung was extremely diseased, and some parts of it as hard as liver. The middle lobe particularly was very hard, and nearly one half of it of a yellowish white colour. This portion consisted of a collection of distinct roundish bodies connected together by a membrane. They were for the most part of a firm consistence, and resembled diseased bronchial glands. Well formed pus was found in the centre of some of them; in others, a substance like cheese curd. This nucleus was partly enclosed by the natural substance of the lungs, and its boundaries well defined. It appeared to me to be a collection of tubercles, which, as they increased in size, had destroyed the intermediate substance of the lungs. I could not believe it to be a recent disease, and it seemed that the child had a severe pneumonia, when it was about a year old. Pus was found in every other part of this lobe, lodged in sinuses, many of which communicated with the cavity, which contained the above nucleus. In the other lobes of this lung much pus was found, and numerous small tubercles.

The left lung seemed little diseased; some tubercles, however, and some pus was found in its inferior portion.

The bronchial glands were generally diseased. They were observed in clusters, and very much enlarged; some of them as hard almost as bone, others of the consistence of firm marrow.

The mucous membrane of the trachea exhibited spots of a dark red colour. These were more numerous in the bronchiæ, and, in the smaller branches, this colour was quite general; and as much so in those, which belonged to the sound, as to the diseased lobes. The bronchiæ were filled with a semipurulent matter of a reddish tinge; the pus seen in the lungs had not found access into them. The mucous membrane of these tubes was particularly noticed to be thickened, and to have lost its natural glossy smoothness.

CASE IX.

S. G. E. aged six years, had had the hooping cough about six weeks. The cough had been severe, but as he had not been otherwise unwell, it was not regarded. February 4th, he began to droop, and his kinks were more frequent and violent. On the sixth, he was suddenly taken with great difficulty of breathing, and a febrile paroxysm. I saw him in the evening; he lay in a state of stupor; breathed laboriously and with great rapidity; face flushed and swollen; skin hot; pulse very small and quick. I took from him immediately six ounces of blood, and applied a large vesicatory to his chest. He had been previously well evacuated by a cathartic.

7th. Symptoms not materially improved. Respiration and pulse much as yesterday. Took from him three ounces of blood in the morning, and three in the evening.

9th. Respiration performed with a little more ease; pulse not improved; coughed more than for two days past, and expectorated florid blood. Urine scanty, high coloured, and loaded with mucus. The most active epispastics had no effect on the skin. Two ounces of blood were taken from him in the morning, and two in the evening.

9th, 10th, and 11th. The symptoms did not improve so much as to afford any encouragement for ultimate success. The respiration continued quick and short; pulse quick and small; florid blood still expectorated. He was bled to the amount of two ounces each day, and he was slightly blistered. After the bleeding on the 11th, there seemed to be a ray of hope; he breathed with tolerable ease; the pulse became less frequent and fuller, and he had a little appetite. This appearance of amendment lasted till the 13th. On the 14th, he was not so well, and continued failing, till the 16th he died.

Dissection.

The body examined sixteen hours after death.—The lungs were of a light colour at their upper, and dark at their under surface from the subsidence of the blood. They had, to the sight, the appearance of being sound; but when pressed between the fingers, numerous small tubercles could be felt in almost every part of them. These were hard, and varied in size from a small shot to a large pea; they were most numerous in the middle lobe of the right lung. This lobe was the most diseased and when cut into, exhibited pus in numerous insulated globules. Pus was discovered in other parts of the lungs, but not so abundantly as in this. The only portion that retained that hardness, which is a common effect of pneumonic inflammation, was a part of the inferior lobe of the left lung.

The mucous coat of the trachea and bronchiæ was in this case not in the least red, but it was evidently thickened, opaque and deprived of its natural glossy smoothness, particularly in the smaller branches of the bronchiæ.

The inflammation in this case appeared to have been almost entirely removed even from the mucous membrane. But the injury, which that membrane had sustained, and the general diseased state of the lungs seemed to me to have been the immediate causes of death. The pus observed in the substance of the lungs must have been the effect of recent inflammation; and I am disposed to have the same opinion as to the existence of the tubercles; they were very numerous and small. The bronchial glands were not diseased.

The information to be derived from these cases, relates, first, to the nature and seat of the disease; and, secondly, to the proper means of cure.

With respect to its seat, it seems to be well ascertained that the principal affection is in the mucous membrane of the trachea and bronchiæ. In all dissections after death by this disease, this membrane has been discovered to be in a morbid state, and though in some instances, other parts have been found to have suffered, to the affection of no other could the peculiar symptoms of hooping cough be so reasonably assigned. Cases 1st and 2d of this paper, were genuine examples of this disease; in these cases this membrane was the only part, in which the effects of previous morbid action appeared.

As to its nature, it is probable, that we shall at present remain in the dark with regard to what constitutes its essence. All that we are able to discover are the effects of inflammation; and to this cause we must attribute all, that is either distressing or dangerous in the disease. From the general aspect of the symptoms, and from the morbid phenomena disclosed by dissection, we infer, that inflammation is uniformly present, and that it makes an essential part of the disease. In this respect it does not differ from other specific diseases, all of which are accompanied with a greater or less degree of inflammation, and their severity and hazard are in proportion to its activity and extensiveness. Whether this inflammation have any thing specific in its character, the morbid changes do not enable us to determine; and as it regards the treatment it is not material, as the same means will control it in either case.

From this view of the complaint, the treatment should consist in the use of such means as are the most effectual in reducing inflammation. Among these, blood-letting, without question, holds the first place. But besides its fitness for this purpose, experience of its utility sanctions its employment. It is not, however, necessary to bleed in every case of hooping cough; but I am persuaded, that even in common cases, there would be infinitely less suffering during the progress of the disease, were this remedy employed, and there is really no harm in taking a little blood from a child, though it has so formidable a look. At the worst it is much less injurious in my estimation than repeated emetics. But with the present weight of prejudice against it, few probably will be disposed to recommend it, except in severe cases. In these, in order to insure its greatest advantage, it must be employed without delay and with perseverance. I would bleed as soon as the cough became severe; and at this time one liberal bleeding would in most cases prevent mischief, and render the disease mild in its progress.—But upon the accession of the febrile paroxysms, and the hurried respiration, which indicate a great degree of inflammation in the mucous membrane; or when the inflammation has extended to the substance of the lungs, and thus produced, in combination with those of the hooping cough, symptoms of pneumonia, this remedy becomes indispensable to the safety of the patient. Under these circumstances we must not be satisfied with a single bleeding, but it must be repeated in liberal quantities, till the symptoms are under control, or we are convinced that amendment is beyond the power of the art.—A timid use of this remedy is to be deprecated. It often happens, that a first bleeding effects no apparent improvement in the state of the disease, when a second or a third would completely accomplish the object. If abandoned after a single trial, the patient looses the best means of relief; the remedy is brought into discredit, and has laid to its charge many of the evils, really the effects of the disease. It is equally bad policy to delay this remedy till every other has been tried. If employed in the last stage of the complaint, it will probably do no good, and will thus get into disrepute, if not with the physician, certainly with the lookers-on. I am persuaded, that in most instances of its failure to relieve this disease, and I might perhaps add other acute diseases, it is to be attributed to its being resorted to too late, or used too sparingly.

Next to blood-letting, antimonial emetics afford us the most efficient means of cure. But, if there be any affection of the head, cathartics, particularly of calomel, seem to have the happier effect.

Blisters are, probably, useful remedies, but they have appeared to me to do less good in inflammations of the mucous membrane, than in pneumonia or pleurisy. An efficient use of the lancet will render their employment rarely necessary in either form of the disease.

Chemical Examination of a quantity of Sugar, supposed to have been intentionally poisoned. By John Gorham, M. D. *Professor of Chemistry in Harvard University.*

[Communicated for the New-England Journal of Medicine, &c.]

IN February, 1817, I received from Dr. Nichols, of Kingston, Massachusetts, about two drachms of common brown sugar, together with a letter, in which he observed, "I was called this day (Feb. 18,) to visit in a family of which every member, except one, was suddenly taken sick, puking every thing swallowed; and they considered it to arise, and circumstances favour the supposition, from eating the sugar of which I send you a sample."* I was requested, by Dr. Nichols, to examine it, and give him some account of the results.

On inspection, small white grains, or particles, could be observed disseminated through the sugar, and, after tasting it, a peculiar acrid impression was left for some time in the mouth. In order to ascertain the nature of this foreign matter, it was first necessary to obtain it in a separate state, and this was effected by putting the mass into a common jelly glass, and nearly filling it with cold distilled water. The mixture was agitated with a glass rod, until the whole of the sugar was dissolved; the solution being then allowed to remain at rest, for a short time, a white, ponderous powder was first observed to subside, after which succeeded a precipitate of a light grey colour. The solution was then poured on a filtre; the solid insoluble matter was washed in repeated portions of distilled water, which were reserved for examination, and the precipitate was collected and dried at the common temperature of the air.

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* The individual who escaped, did not use the sugar.

Its colour was a light gray, or dirty white, and it weighed one and a quarter grain. White particles could be perceived in it, and the shade of colour was owing, as I afterwards had reason to suppose, to the impurities of the sugar. Allowing one quarter of a grain for the weight of this grayish powder, the amount of the white substance may be estimated at *one grain*. This was divided into eight parts, and, for the sake of greater precision and convenience, these parts were respectively marked Nos. 1, 2, 3, &c. to 8. My first object was to ascertain, whether this powder consisted of, or contained, arsenic.

Experiment 1. No. 1 was put into a watch glass, with one third of a grain of pure solid potash, twenty drops of distilled water were added, and the whole was boiled to dryness. Upon the solid mass, about twenty drops of distilled water were poured, and by agitation the greater part was dissolved; but the solution was turbid. On allowing it to stand for a short time, a grayish powder subsided, and the clear liquid was decanted. A drop of this solution, added to a watery solution of sulphate of copper, produced a distinct precipitate of a *grass green* colour. When added to the amount of six drops, the precipitate was abundant. The solution of sulphate of copper, although not saturated, was of considerable strength.

Experiment 2. One eighth of a grain of arsenic of commerce, was mixed with about a third of a grain of solid potash, and treated in the same way, viz. by mixture with water, boiling to dryness, and subsequent solution in twenty drops of distilled water. One drop of this solution, added to another portion of the same solution of sulphate of copper, occasioned a *grass green* precipitate; six drops produced a copious deposition, and the colour and appearance of this substance so closely resembled those mentioned in experiment 1, that the eye could distinguish no difference between them.

Experiment 3. About a drachm of solution of nitrate of silver was poured into a glass; one end of a clean glass rod was dipped into a solution of pure ammonia, recently prepared, and the drop adhering to it was brought into contact with the metallic solution; the other end of the rod was then immersed in the liquid, as prepared in experiment 1, and afterwards made to touch the surface of the solution of silver; a dense *yellow coloured* precipitate was immediately formed. When three drops each of the liquid above mentioned and of solution of ammonia, were added to the nitric solution of silver, the precipitate was considerable.

Experiment 4. The same quantity of solution of nitrate of silver being taken, a drop of liquid ammonia, and afterwards one drop of a solution known to contain *arsenic* and potash, or arsenite of potash, were added. A dense *yellow coloured* precipitate instantly took place, precisely similar, in every respect, to that produced in experiment 3.*

Experiment 5. The yellow coloured matter, (experiment 3,) was repeatedly washed with distilled water, dried, mixed with thrice its weight of powdered charcoal, and introduced into a small thin glass tube closed at one end, the other being afterwards obstructed with a roll of paper. The part of the tube, containing the materials, was then immersed in the flame of a spirit lamp; it soon became red hot, and was allowed to remain in that situation for ten minutes. When it became cool, no metallic film was observed, but the internal surface, about an inch from the sealed extremity, was thickly studded with a *white, crystalline,* and apparently granular *sublimate.*

Experiment 6. The yellow coloured precipitate, (experiment 4,) having been increased in quantity, by the addition of three or four drops each of the ammonia and the arsenical solution, to the solution of nitrate of silver, was collected and managed precisely as in experiment 5. When the tube was taken from the lamp and cooled, no metallic film was visible; but the internal surface exhibited a *white, crystalline,* and apparently granular *sublimate,* differing in no respect from that of experiment 5.

Experiment 7. No. 2 of the white powder was mixed with about three times its weight of charcoal powder; the mixture was put into a thin glass tube, about a line in diameter, and hermetically sealed at one end. The mouth was closed with a roll of paper. The lower end of the tube was then exposed to the flame of a spirit lamp; it soon became red hot, and was kept in that state ten minutes. After being cooled, its internal surface, about half an inch from the sealed end, exhibited a distinct *metallic film* of a *bluish* colour.

Experiment 8. One eighth of a grain of powdered arsenic of commerce, was mixed with thrice its weight of charcoal powder, and put into a tube similar to the one above mentioned.

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* As the arsenic in experiments 3 and 4, was previously combined with an alkali, the addition of ammonia to the solution of nitrate of silver, was, perhaps, superfluous. This circumstance did not occur to me, until after the experiments were performed; but it obviously had no influence on the results, as the ammonia was not added in sufficient quantity to re-dissolve the precipitate.

It was exposed in the same way, and for the same length of time, to the flame of the spirit lamp. After the experiment, its internal surface was partly covered with a *metallic film* of a *bluish* colour; and if there were any difference between this, and that of experiment 7, the latter was rather more distinct.

Experiment 9. No. 3 was put into a watch glass, twenty drops of pure muriatic acid were added, and they were exposed to heat, until the greatest part of the powder was dissolved. A grayish powder remained, and this appeared to consist of the impurities of the sugar. It was too minute in quantity to admit of a satisfactory examination, and I did not think it essential. A watery solution of sulphuretted hydrogen was then made, and a few drops of this being added to the muriatic solution, occasioned the formation of a fine *straw coloured* precipitate. When this substance was collected, dried, and exposed on a spatula of platina to the heat of a lamp, it first turned red, seemed to melt, exhaled a sulphurous odour, and passed off entirely in vapour.

Experiment 10. One eighth of a grain of arsenic of commerce was dissolved in muriatic acid, solution of sulphuretted hydrogen employed in experiment 9, being then poured into it, a *straw coloured* precipitate immediately formed, which, when dried and exposed on platina to heat, first turned red, then melted, exhaled a sulphurous odour, and was dissipated in vapour.

A fragment of native orpiment, or the yellow sulphuret of arsenic, exhibited, on being heated over the lamp, precisely the same phenomena; its colour changed to a red, it then became liquid, exhaled the odour of sulphurous acid, and was entirely dissipated in vapour, leaving no residuum.*

Experiment 11. No. 4 was taken upon the end of a penknife, and held over the flame of a lamp; a white vapour was soon perceived to rise from it, which had the peculiar alliaceous odour, characteristic of arsenic, and the white oxyde in the elastic form.

No. 5 was unfortunately lost in an attempt to obtain arsénic acid. It was mixed with nitric acid, contained in a glass capsule; but on exposing it to heat, in a bath, the vessel broke, and the materials disappeared in the sand. The object of the experiment was to form the red arseniate of silver, by adding the arsénic acid to an ammoniated solution of nitrate of silver.

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* When the sulphuret of arsenic is exposed to heat, the sulphurous odour predominates so much, that the arsenical odour is scarcely perceptible.

Experiment 12. No. 6 was mixed with twice its weight of powdered charcoal; the mixture was put between two polished plates of copper, secured by iron wire, and exposed to a red heat for ten or twelve minutes. When the plates were cool, they were separated, and a whitish mark, or, in other words, a whitish alloy was found to have been formed on the surfaces, which had been in contact with the mixture.

Experiment 13. One eighth of a grain of arsenic of commerce, was mixed with twice its weight of charcoal; the mixture was put between two similar copper plates, secured by wire, and heated red-hot for ten or twelve minutes. When cooled and separated, their internal surfaces exhibited precisely the same appearance as the plates in experiment 12.

Nos. 7 and 8 were expended in repeating, with the utmost care and attention, the first, third, and fifth experiments above mentioned. The results were uniformly the same, and I had no reason to doubt of their accuracy. They have been faithfully detailed, in the order in which they were made.

Observations.

The results of these experiments are unequivocal. The white matter, mixed with the sugar, displayed all the chemical characters of the arsenic of commerce, the white oxyde, or arsenious acid of chemists. The first, third, fifth, and seventh experiments were amply sufficient to prove this fact; but having enough of the powder to complete a more extensive series, I also performed those which may be considered as of more doubtful character; such as the whitening of copper, the vaporization and ascertaining the odour of the vapour, and the formation of a sulphuret of a light yellow colour, by the addition of sulphuretted hydrogen to the muriatic solution. These properties alone would not be sufficient to warrant an unqualified opinion of the nature of the substance under examination; but when taken in connexion with the results of the other experiments detailed, they mutually confirm each other, and enable us to arrive at certainty on this subject. The most important phenomena which arsenic exhibits, and which, with the least danger of mistake, may be regarded as the most delicate tests of its presence, are its union with the oxyde of copper, and forming with it an insoluble substance of a fine green colour; its combination with oxyde of silver, which produces a yellow coloured compound, and its reduction to the metallic state, by being mixed and heated with black flux, or powdered charcoal.

The action of arsenious acid, on the salts of copper, has been known from the time of Scheele; he discovered the mode of forming this pigment, and it is often called Scheele's green. It is formed by a double affinity. The oxyde of arsenic, or arsenious acid, is previously combined with potash, and forms with it an arsenite of potash; when this salt is added to solution of sulphate of copper, the sulphuric acid of this salt combines with the potash of the one added, forms sulphate of potash, which remains dissolved in the water; the arsenious acid and the oxyde unite to form an arsenite of copper, which is green, and being insoluble, is separated. The salts of copper are delicate tests of the presence of arsenious acid, for, when so much diluted as to be nearly deprived of colour, a drop of a liquid, containing arsenic, will produce distinct streaks of a green colour. The arsenious acid is previously combined with potash, because its solubility is increased, and, at the same time, it is rendered more susceptible of decomposing cupreous salts. This mode of detecting arsenic is one of the best which have yet been discovered.

The test of Dr. Marcet, the nitrate of silver, is equally delicate, and it is capable of discovering a very minute proportion of this poisonous substance. A drop of ammonia is added to solution of nitrate of silver and then a drop of the liquid supposed to contain arsenic; if the supposition be true, a yellow insoluble arsenite of silver is formed. When the arsenious acid is combined with an alkali, as in arsenite of potash, nothing is gained by the addition of ammonia to the nitric solution of silver; for the potash saturates the nitric acid, while the arsenious acid combines with the oxyde.

But it has been found that there is another substance which is capable of forming a yellow coloured precipitate with oxyde of silver, so closely resembling the arsenite of silver that they are distinguished from each other by the eye with great difficulty. This is the phosphoric acid. An alkaline phosphate and phosphoric acid alone when ammonia is present, will produce this kind of precipitate in nitrate of silver; and it is therefore necessary to investigate the properties of the yellow precipitate in experiments to detect arsenic, in order to ascertain whether it be phosphate, or arsenite of silver. This necessity may sometimes render the use of this test inconvenient, and, where the quantity obtained is very small, even doubtful, from the impossibility of arriving at accurate results in operating with minute portions of matter. If, however, the same substance should not only occasion this precipitation in solution of nitrate of silver, but also the formation of Scheele's green in solution of sulphate of copper, no doubt can be entertained respecting its nature.

According to Dr. Marcet, the phosphate of silver, when enclosed in a tube and exposed to heat, yields no smoke, nor crystalline sublimate; when urged on charcoal before the blowpipe, it forms a vitreous, greenish coloured and difficultly fusible globule [*] The arsenite of silver is characterized by very different properties. It grows brown on exposure to light; it is soluble in nitric acid and in excess of ammonia; it is decomposed by heat, and when the process is conducted in a tube, a white vapour ascends which condenses on the colder part in the form of minute octaedral crystals.[†] It is this decomposition which Dr. Marcet has considered as the *Experimentum crucis*, in detecting the presence of arsenic in the yellow precipitate; the white sublimate being arsenious acid. Hence, I was led to perform the experiment with great care, and the result exhibited in the most satisfactory manner, that the yellow precipitate, (Exp. 3,) was not phosphate but arsenite of silver. I have stated in Exp. 5, that the white crystalline sublimate was apparently granular, for the crystals were so minute that their actual forms could not be distinguished without the aid of a microscope, and, as I thought it possible that the tube might be required, the contents were not removed. The strongest evidences, therefore, which can be obtained of the presence of arsenic, existed here; these evidences are, the formation of Scheele's green, (Exp. 1;) the production of arsenite of silver, (Exp. 3,) the decomposition of this arsenite, (Exp. 5,) and the reduction of the arsenic to its metallic state, (Exp. 7.) This powder discovered an union of properties which belong only to arsenic, and admit of an uncontrovertible opinion on the subject.

I employed water to separate the sugar from the white powder, because, from the comparative slowness with which the metallic salts and oxydes are dissolved, it was presumed that the whole of the sugar would be rendered liquid, before the weight of the other substance could be sensibly diminished. This was the fact; for the solution of sugar exhibited no trace of arsenic, nor was any sensible quantity dissolved by the water, with which it was washed. In fact, the proportion of arsenious acid which water at common temperatures is capable of taking up, is very small; for it was found by Klaproth that 1000 parts of the former at 60° dissolved only 2 1-2 parts of the latter.*

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[*] Medico-Chir. Trans. vol. VI. p. 66 3-4.

[†] Nicholson's Journal, vol. xxxiv. p. 174.

Some persons, perhaps, may object to the conclusion which has been drawn, from the smallness of the quantity submitted to experiment. The results which may be regarded as decisive and which are fully sufficient to justify an unqualified opinion on the nature of the substance, were obtained from *one eighth of* a grain in weight; but they were so obvious and unequivocal, that they inspired as much confidence as if they had followed experiments on one eighth of an ounce. That it is not necessary to operate on large quantities, in order to be assured of the correctness of the conclusions, has been shown, among others, by Dr. Marcet, who proved, that a child had been poisoned by arsenic, by examining the liquids ejected from the stomach, in which he infers that no more than one tenth of a grain could have been dissolved.†

It has been objected, that the test of nitrate of silver will not answer, when muriatic acid is present with arsenic. This objection would be valid, were there no means of separating the arsenite from the chloride of silver. It is true, that when muriate of arsenic is added to nitrate of silver, the precipitate formed, instead of being yellow, is white, even when ammonia is present. But the arsenious acid may be discovered by the following process of Dr. Marcet.

"To the suspected fluid, previously filtered, add, first, a little diluted nitric acid, and afterwards nitrate of silver, till it shall cease to produce any precipitate. The muriatic acid, being thus removed, whilst the arsenious acid (if any, and in whatever state) remains in the fluid, the addition of ammonia will instantly produce the yellow precipitate in its characteristic form. It is hardly necessary to add, that the quantity of ammonia must be sufficient to saturate any excess of nitric acid which the solution may contain."‡

A Case of Alarming Hæmorrhage, from the Extraction of a Tooth. By W. Channing, M.D.

[Communicated for the New-England Journal of Medicine, &c.]

MR. A. L. about 40 years of age, had previously to the occurrence to be presently mentioned, lost a number of teeth. They had in most instances become loose and were almost spontaneously removed from their sockets. The last but one of the molares on the right side of the lower jaw, was perceived to be loose on the 28th of May, and on account of some slight inconvenience to the patient, he at once extracted it with his fingers. This tooth was externally sound, and only differed in appearance from those formerly removed, in having lost some of the natural smoothness of the prongs, and in having the membrane between them considerably thickened.

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* Annals of Philosophy, vol. IV. p. 133.

† Medico-Chir. Trans. vol. VI. p. 664.

‡ Nicholson's Journal vol. xxxiv. p. 177.

The tooth was removed at about 3 o'clock P. M. and a large hemorrhage immediately succeeded. The patient had lost a good deal of blood from removing a tooth at a former period, and was not alarmed at the quantity he was now losing. I saw him between eight and nine in the evening, and found that he had lost full two quarts of blood. From the gradual manner in which the blood flowed, and the recumbent position of the patient, little faintness had been induced, although he was suffering at times, and particularly once on rising, very severe chills. An examination was made of the tooth. The two processes, by which it had been fixed into the jaw, were quite short, and almost exactly on a line with the body and crown. The patient washed his mouth with a strong solution of the sulphate of zinc, and sulphate of iron, but it was found to be immediately filled with arterial blood. On clearing the mouth, and applying the fingers firmly to the gums, at the place from which the tooth had been removed, I was enabled to ascertain the violence of the hemorrhage, and, at the same time, to convince myself, that it could be commanded. Ice was now applied freely to the side of the head, face, and neck. It was introduced into the mouth, and applied to the bleeding part, while firmly compressed by my fingers. The sulphate of zinc was applied freely in powder to the part, and this was alternated with the sulphate of iron. These means were, however, found to be merely temporary in their effects; when pressure was removed, the hemorrhage became at once as urgent as before their use. A plug was now made of fine linen, longer than the tooth, and firmly wound round with thread. This, with some difficulty, was pressed into the hole from which the tooth had been removed, and was preserved in its place by pressure from the tooth immediately above it. For some time this, in some good degree, answered the objects had in view, in its application; but it was found but imperfectly to occupy the place into which it had been pressed, and the hemorrhage was renewed when the patient spoke, or inconsiderately moved the tongue to the part, to which he was constantly inclined. I requested the attendance of a medical friend, who had devoted many years to the diseases of teeth, and to the accidents to which their extraction is occasionally liable. We concluded, on consultation, that the hæmorrhage might be checked by forcing a pellet of cotton into the socket. The yielding nature of this material seemed to promise success, from its more accurately filling an irregular cavity, than the plug. The attempt was made, but the soft parts were so very flaccid, and the hæmorrhage so great, that the cotton was forced out at every attempt to fix it. Recourse was again had to a plug. A new one was immediately introduced, and to prevent the possibility of its being removed by the tongue, or motion of the jaw in spitting, a waxed thread was passed round it, and attached to a neighbouring tooth. The hæmorrhage was now checked, and at 11 at night we left the patient comfortable, if we except pain and stiffness of the cheek, which were the consequence of great swelling, which rapidly took place at the close of our attempts to check the flow of blood.

The bleeding had not returned on the morning of the 29th. Except a considerable degree of weakness, the patient was very comfortable. On the morning of the 30th, the plug was found to be quite loose, and the hæmorrhage checked. The plug was removed. It was found perfectly white; not at all discoloured by blood. A small portion of coagulated lymph was observed on its lower end. The blood, in this case, *coagulated*. The tooth was carefully dissected, and its vessels found shrivelled and impervious.

Case of Spina Bifida. By Pliny Hayes, M. D.

[Communicated for the New England Journal of Medicine, &c.]

On the 28th of March, Mrs. N. was delivered of a female child, with spina bifida. It moved the inferior limbs with facility, but appeared to possess no power of controlling the evacuations. From the 2d to the 14th day, it was frequently convulsed. At the suggestion of a medical friend, the tumour was punctured on the 18th day, and its contents, which consisted of a limpid fluid, partially evacuated. The operation was repeated on the 20th, and again on the 24th, when the internal surface of the sac, constituting the tumour, was pricked with the point of the needle, and, after the fluid was evacuated, a compress and bandage applied. The emaciation, which commenced at birth, had now become so great, that the parents objected to any further attempts to remedy the disease. On the 34th day, however, the tumour was again punctured, the internal surface of the sac irritated with the needle, the fluid evacuated, and a pad applied; but no adhesion or inflammation followed. The punctures, except the first, were made with a round pack-thread needle, half a line in diameter. They always healed, so as not to be visible on the 3d day. About this time, manifest signs of water in the head were perceived. The child lingered till the 62d day, and died.

The alvine evacuations had uniformly been green, more or less inclining to black, and frothy, and took place, as well as the urinary discharge, whenever the child cried, or was disturbed. It was generally very restless; but a grain or two of calomel, which was occasionally given, had always the effect of composing it for a few hours, and rendering the alvine discharges more natural. A pulsation was once distinctly noticed in the tumour.

Dissection.

In all the vertebræ of the sacrum, that portion which should form the posterior part of the vertebral canal, was entirely wanting. Below the lumbar, vertebræ, the dura-matral sheath of the spinal marrow, was protected posteriorly by the common integuments only, with which it was firmly united. It formed here a sac, which contained the cauda equina and the fluid before mentioned, and constituted the tumour. The spinal marrow entered the superior extremity of the sac entire, but was immediately bifurcated, and, after giving off considerable nerves through the inferior lumbar and superior sacral foramina, ramified over the whole internal surface of the sac, but principally on the lateral parietes, giving off filaments from numerous points which converged to pass through the holes of the sacrum into the pelvis. The sac terminated about half way down the sacrum. The vertebral canal was not more than half filled by the spinal marrow. The vertebræ of the sacrum were cut through, to demonstrate that they contained no cavity. The fact, that the sphincters and contiguous muscular organs were paralized, while the inferior extremities were moved at will, may be readily explained by a reference to the different origins of the nerves, which supply the respective parts.

In the ventricles of the cerebrum were found four or five ounces of fluid, more than half of which consisted of thick pus. The contiguous medullary substance was discoloured, and of a weak and somewhat gelatinous texture. Situated above the lateral ventricles were found two preternatural cavities, one in each hemisphere, extending nearly the whole length of the middle and posterior lobes of the cerebrum. Their internal surfaces were sound, in some places exhibiting distinctly the fibrous structure of the medullary substance. They contained a very little fluid, without any pus. The cerebellum, and all the parts in the base of the skull, were sound. The subject was emaciated to the last degree. The ossification of the cranium was very imperfect. Many other singular appearances of less importance were noticed.

Case of peculiar formation of the Umbilical Cord. By Dr. John Mc'Nabbe.

[Communicated for the New England Journal of Medicine, &c.]

ON the 22d of August, 1816, I was called to attend a lady in travail. She was delivered of a feeble male child. When about to divide the funis umbilicalis, I was astonished, indeed, on beholding a singularity, which, to me, was a new thing, and entirely out of the common course of nature. The navel was about four inches long, and remarkably large and hard. The funis appeared like two, adhering to each other, and attached to the upper and lower edges of the elongated navel. About three inches from the navel, the apparently double cord united more closely, and became *one*, of a remarkable magnitude. I divided it, and applied the ligature in the usual manner. About the fourth day after delivery, I was called upon to visit the child, who appeared to be in acute pain, which was much increased on motion. I directed the nurse to examine the dressings on the navel, and see if they caused the distress. There did not appear to be any remediable difficulty, and the cord, or cords, were almost separated from the navel. In two days more, a complete separation took place. The child was still in distress, and the extremity of the navel, assumed an inflammatory and very disagreeable appearance. Two other physicians of respectability were called to see the child, who declared they had not seen any thing like it in their extensive practice.

The next day I visited the child, and was informed by the nurse and other attendants, that it had voided considerable urine at the lower edge of the navel, exactly where the lower cord was attached; after which evacuation it appeared to be considerably relieved. The navel is not so long now as formerly, nor does it exhibit such a disagreeable appearance; yet it is very troublesome. Its extremity very much resembles the glans penis. Suitable dressings are still necessary to defend it from external injury, to which it is very liable, and frequently receives from its apparel, it being very tender. I am not able to determine with certainty, but am induced to believe, that there were arteries in each branch of the fork. Although there was an apparent union of the branches at a small distance from the navel, yet my opinion is, that there were proper and distinct blood vessels for two cords. The ground of my belief was the appearance on the dissection, which I made of the cord or cords near the placenta, and of the placenta itself, about thirty minutes after delivery. The placenta was very large, and appeared like two, firmly adhering to each other; likewise the appearance of the cords at the placenta very much resembled that at the navel. The child discharges its urine by the urethra freely, and in usual quantity for one of its age. There has been a daily evacuation of urine from the navel, ever since the time it made its first appearance there.

Once in about four or six days the child is unwell, and seems to have considerable pain in the umbilical region, until a copious discharge issues therefrom, after which it appears to be well and perfectly easy. I was informed by its mother, a few days ago, that the evacuation from the navel increases considerably in quantity, without producing any diminution in the discharge by the urethra.

Notwithstanding all the attending difficulties, the child grows very well. Its head is very large, and it has had a cough ever since it was born.

It had the influenza last November; since that time, the cough has been more severe. It has, ever since its birth, had much difficulty in swallowing, either solids or fluids. I believe there is some imperfection in the organs of deglutition.

Barnet, (Ver.) June 10, 1817.

The foregoing is an instance of the urachus being open to the umbilical cord. The urachus is a sort of ligament extending from the upper part of the urinary bladder toward the umbilicus. It has a small cavity communicating with the bladder, but not usually remaining open, as far as the umbilicus. In the fœtus of quadrupeds, there is an open urachus, which extends from the bladder to the allantois, one of the membranes which invests the quadruped fœtus. This allantois is supposed to be a temporary receptacle of urine, conveyed to it from the bladder through the urachus. Cases of open urachus in the human fœtus are not common. Ed.

Effect of Fright. By Henry S. Waterhouse, M. D.

[Communicated for the New England Journal of Medicine, &c.]

MRS. A——, aged 40, who had borne eleven children, unattended by any unusual symptoms or circumstances, whilst pregnant of her twelfth child, and near the termination of the seventh month, underwent a very violent fright. A drunken brute of a fellow attacked her husband, who was driving the sleigh in which she rode, dragged him by the collar out of the sleigh, and began to beat him. During the squabble that ensued, Mrs. A. exerted herself (by words only) to dissuade them from fighting. She soon felt that her breathing was difficult and laborious—had severe shiverings, and experienced occasionally the sensation of cold water running down her back. Coldness at times often for the seven or eight days following, and faintings whenever she faced towards, and was near the fire, till the time of her delivery. When she sat her back towards the fire, disagreeable chills affected her.

Before this time the movements of her uterine burthen, had been felt as is common; but, after this, she experienced no more motion.

On the 2d of March, Esquire A. requested me to visit his wife. I called on Doctor Morse, and we saw her together. She described to us her symptoms as above, the circumstances of her fright, and the sensible effects. She assured us, that the size of her abdomen was diminishing as rapidly as it had ever before increased at the same stage of pregnancy. This progressive diminution of bulk continued, till the time of parturition.

We advised her to remain quiet, and to make no use of medicines, unless symptoms should occur, which might require the aid of remedies.

March 28th. When she first waked in the morning, she felt much soreness at the upper part of each inguinal region. This continued till the following morning, when, instead of soreness, she had pains at regular intervals, in these parts, across the lower and anterior part of the abdomen, and low down into the same side of the pelvis. She experienced no sensations in her back, save those of extreme weakness. The membranes burst, and the waters were discharged soon after 12 o'clock, on the morning of the 30th. I could not see her, till 3 o'clock of the following morning. The child was born when I arrived, and the placenta came away in due time. Her after-pains were somewhat less than was common for her to undergo; and the discharge of blood from the uterus, consequent to labour, was but trifling. I did not discover any circumstance unfavourable to her recovery.

There was not the smallest appearance of monstrosity, or malformation about the child. It was a male, and perfect in every respect; I mean, as far as its age of utero-gestation would permit us to expect. The length of its head, neck, trunk, and extremities were equal, I believe, to that of any common fœtus, at or near the conclusion of the seventh month of uterine growth; but it was, in all parts, *emaciated* to an astonishing degree. It weighed only one pound and one ounce avoirdupois. Its thighs were not larger than the first phalanx of the fore-finger of a middle-sized man. The scalp was loose and wrinkled; it seemed to be not more than two thirds filled by the head. The skin of its face and neck was of a loose pale red colour; that of its trunk and extremities, of a pale green. The cuticle was, in many places, detached from the cutis vera, and slipped easily on it, though there was not the smallest fetor emitted from any of the contents of the uterus. The placenta seemed to be perfect and healthy; it was much less spongy, that is more solid, of course much thinner, than is common. The side, detached from the uterus, had every look of recent separation.

That extremity of the funis, connected with the placenta, was small and semi-transparent; but it became larger, as it approached the fœtus; several inches of this extremity was over an inch in diameter, and was stuffed with dark fluid liquid.

The liquor amnii was discharged, whilst Mrs. A. was sitting in a chair; the quantity was such as to wet through her clothes, the cushion of the chair, and formed a small puddle on the floor. All the blood discharged during, and immediately succeeding her labour, did not exceed six ounces. About one third part was fluid, the remainder was in coagula; both had every appearance of fresh blood.

That the fright and agitation of mind, which Mrs. A. underwent, caused the extinction of life in the fœtus, cannot, I believe, admit of a doubt; but, by what peculiar process of the animal economy, this was brought about, is not, I suspect, easily explained. Want of circulation, probably, from paralysis in the umbilical vein, seems to have been a near cause of death; but how fright of the mother should produce this paralysis, must remain for abler physiologists to develope.

Another curious question presents itself for physiological disquisition: By what mode of action, or process of the absorbent system, was the bulk of the fœtus, and consequently the size of the mother, so rapidly and greatly diminished? The umbilical arteries, it would seem, had much agency in this phenomenon. Is it not very possible, that these arteries, after they had ceased to perform their proper arterial functions, might serve as tubes, or canals, from which the maternal absorbents of the placenta might derive fluids, and convey them into the lymphatic system of the parent? The amnian liquid could not have been much, if any, diminished in quantity. There can be no doubt, that the placenta was detached only a short time previously to the expulsion of the child.

Can this case serve, in any degree, as a clue to determine the long agitated question, concerning the manner in which vitality and nourishment are derived to the fœtus?

Malone, N. Y. May 28th, 1817.

Case of a Foreign Substance lodging in the Lungs. By J. B. Brown, M. D.

[Communicated for the New England Journal of Medicine, &c.]

C. B. the son of a gentleman of this town (Boston) thirteen years old, swallowed a white tack, or pump nail, Feb. 1814. Soon after, he had a slight cough and occasional difficulty of breathing, which was always imputed to a cold, and excited so little alarm, that his parents did not consult a physician until March, 1817. I was then sent for, and found the lad apparently attacked with pneumonia. He was bled and blistered, which gave him relief, and in the course of three weeks, he was able to return to his school in the country, where he had been for nearly two years. About the latter part of April, he sent word to his parents, that he had got rid of the tack, and, at the commencement of his vacation in May, he brought it home with him. The account he gave me is this. That he raised some blood, soon after which, he had a fit of coughing; and feeling something hard in his mouth, he, with his fingers, took out this nail or tack. I examined it, and found about one third of it, in the state of a black oxyde, which was separated from the rest, by handling. The remaining part of the nail retained its usual form, but was discoloured. Since this occurrence, I have been told of a similar accident, that happened to a child in the town of Wilmington, Massachusetts. It swallowed a melon seed, and thirteen months after, in a violent paroxysm of coughing, its mother gave it a slap on the back, and the seed was forcibly expelled from the mouth. This child was afflicted with a cough and affection of the chest, during the retention of the seed, which entirely left it after its ejection. I find in the New England Journal, vol. II. page 89, an account, which John Mervin Nooth, M. D. F. R. S. gives of his own case. He swallowed a leaden shot, in the last glass of a bottle of wine, and ejected it thirteen years afterwards, in a paroxysm of coughing. For the two last years, he laboured under a violent affection of the chest, which, at length, had arisen to such a height, as to threaten immediate death. His symptoms were alleviated by the ejection of the shot, and he soon got well.

From the consideration of the above cases, is it not natural to conclude, that phthisis pulmonalis arises frequently from foreign substances, producing irritation, inflammation, and, finally, ulceration of the lungs? There can be little doubt, that these cases would all have terminated fatally, had not the substances been fortunately expelled.

May 26th, 1817.

[To the Editors of the New England Journal of Medicine, &c.]

Gentlemen,

IN the fifth volume of your Journal, I communicated a case of diseased foot, with an account of the amputation of a part of it, by sawing through the bones. I stated at that time, that the cure was, probably, retarded by the disease having been suffered to continue so long, and that I had but little doubt, that if the same operation was performed, where the disease was recent, that the parts would speedily heal. Since that time, I have had an opportunity of performing the operation, precisely in the same way, excepting that the metatarsal bones were sawed through, instead of the cuneiform. The patient was nearly sixty years of age, and addicted to an irregular and intemperate mode of life. The disease, however, which originated from frost, was recent, of only a few weeks standing. For the first three days succeeding the operation, the appearances of the stump were unfavourable, and the discharge consisted of an offensive, bloody ichor; there was, also, some sloughing of the integuments. A liberal use of wine and bark in substance, produced a healthy discharge, and, in seven weeks, the stump was healed, so that the patient could walk on it with great ease. At that time a very small opening was made in it, which lasted two or three weeks, and was occasioned by the patient's striking it in walking incautiously. There were no unpleasant symptoms, and no appearance of exfoliation during the cure, and that foot is now nearly, if not quite as useful as the other. As this operation can be performed so much more expeditiously than that of disarticulation, and in many cases where the disease will not admit of removal at the joints, and as it can be done with so much more ease to the patient as well as the surgeon, I can see no reason why it should not, in every case, be substituted for it.

I am, gentlemen, with respect,
your humble servant,
GEORGE HAYWARD.

June 29th, 1817.

Note. The English surgeons usually practice the operation of sawing the bones of the foot, while the French prefer separating them at their articulations. The former operation is the most easy to be performed, the latter is the least liable to be followed by exfoliation. ED.

On an easier mode of procuring Pure Potash, than those now adopted. By J. FREEMAN DANA, M. D.

[Communicated for the New England Journal of Medicine, &c.]

I HAVE often observed, that the slag remaining above the button of metal, after reducing ores by the black flux, is very caustic to the taste; that it deliquesces rapidly in the air, and that it slightly effervesces with acids; it occurred to me, that these phenomena were owing to the presence of pure potash, and that it was highly probable this substance had been formed from the carbonate contained in the flux, by the presence of carbonaceous matter, aided by a high temperature, in the same manner as the pure earths of Barytes and Strontites are obtained from their respective carbonates, when heated in contact with charcoal powder, viz. by the formation of carbonic oxyde, which does not combine with the earths.

If these conjectures be true, it is evident, that to procure pure potash, we have only to expose the potash of commerce to heat in contact with charcoal, dissolve in alcohol, and distil off in the usual way.

I exposed a quantity of black flux, made from flour and nitre, to a strong heat in a Hessian crucible for one hour; when taken from the furnace, the crucible was broken, and the fragments, with the adhering fused mass were placed, while warm, in rectified alcohol, and suffered to stand two days; the alcohol assumed a reddish hue; it was then distilled, in a glass retort, until the black carbonaceous pellicle appeared; the liquid was then transferred to a crucible of pure silver, and evaporated nearly to dryness; the contents of the crucible became solid when cold, and were of a lighter colour than the pure potash usually procured for chemical purposes, intensely caustic to the taste, acting with great energy on the skin, producing a high temperature, when placed in contact with a few drops of water in a test tube; some portions slightly effervesced with acids, and some produced no effervescence; in fine, it was pure hydrate of potassa.

I am not aware, that this mode of obtaining pure potash has ever been employed by chemists, it is certainly a much more expeditious mode than that by means of lime, and much less expensive.

Cambridge, July 3d, 1817.

On a Substitute for Alcohol in making Anatomical Preparations. By Mr. William Cooke, Surgeon.

[From the London Medical and Physical Journal.]

WITH a practitioner in medicine, anatomy and physiology should always be directed to the improvement of pathology.

There are, unhappily, great impediments to the cultivation of this part of medical science, derived chiefly from the prejudices of mankind against what is accounted a mutilation of the body; and no arguments will generally avail to counteract this repugnance to opening the *dead*, but those that are dictated by a mind deeply impressed with the advantages which accrue to the *living* from such investigations.

Whenever practicable, the preservation of specimens of natural and morbid structure contributes greatly to enhance their value, by making them not only objects of occasional reference to the primary possessor, but, when he ceases to exist, they are handed down, and perpetuate instruction to posterity.

The expense of glass and spirits has deterred most practitioners from accumulating specimens of this nature, and therefore every suggestion designed to remove the obstacle is worthy of consideration.

During some years, I have been accustomed to preserve diseased parts, and, although on a very circumscribed plan, it has been sufficiently extensive to enable me to judge tolerably well respecting the comparative effects of different preserving media.

The object of this paper is to lay before the profession a few recent experiments made with a solution of the *solid* salt, a purer muriate of soda than the salt of commerce. I shall detail the facts as accurately as I can; for, notwithstanding the success of these experiments has been such as to induce me to give them publicity, I neither wish to excite unwarrantable expectations, nor to pledge myself for the result of more extended essays. It is but equitable, however, to claim the usual concession to first attempts, especially as it was requisite to deviate, in various circumstances, from the common methods of preservation.

Last summer I had the pleasure of introducing Phillips London, Esq. proprietor of the British solid salt, to the Hunterian Musæum. Whilst viewing that splendid exhibition, Mr. London suggested the trial of his salt, which had been found much more effectual in *pickling* meat than other salt; and solutions of which, he had ascertained, would preserve the natural appearance of fish. He soon afterwards politely furnished me with a quantity for that purpose, of which the dates and results are as follow :—

No.	1816 June	*Time of Preservation.*	*Present Appearance.*
1	18	A portion of stomach, from a young man poisoned by arsenic.	The inflamed condition of the stomach, and enlargement of the mucous glands, the same as when taken from the body, but within about the last month a few gaseous bubbles have escaped. The transparency of the fluid continues, although it is not perfectly bright.
2	do.	A small hydrocele, injected with size and vermillion; but the injection did not run well, and the testicle was a little soiled in handling.	In good preservation, one small bubble now resting at the top—fluid clear and colourless; but, when agitated, a few very small flocculent substances are seen in it. The colour of the injection unchanged.

No.	1816 June	*Time of Preservation.*	*Present Appearance.*
3	18	A portion of duodenum, to shew the valvulæ conniventes, and some scrofulous mesenteric glands.	In excellent preservation—the fluid bright, and both the natural and morbid appearance of the parts well retained.
4	24	Part of the stomach of a young man poisoned by opium.	Fully retains its morbid character; but some bubbles of gas have escaped, and are entangled at the top between the talc and specimen—liquid colourless and bright.
5	do.	Large portion of the heart, to exhibit an open and reticulated foramen ovale in the adult.	In excellent preservation, but appears somewhat hardened. The fluid colourless and transparent, although not so bright as some others.
6	do.	Some tuberculated intestine.	Well preserved.
7	Aug. 20	Uterus and appendages.	In perfect preservation—fluid colourless and transparent.
8	Sept. 10	A portion of stomach acted on by the gastric juice, and imperfectly injected with size and King's yellow.	As perfect and bright as possible—colour of the injection unimpaired.

The eye, in different states, has been in the solution from the earliest periods of these experiments; but, not having by me any proper sized bottles, they were not put up. One, however, which has been in the fluid upwards of seven months, to ascertain its effects on the vitreous humour and lens, now stands before me. It retains its natural figure and consistence. The membrana hyaloidea is only affected in its transparency just enough to distinguish it from the circumjacent fluid. The lens has a yellowish opake circumference, yet sufficiently penetrable to exhibit a transparent centre. By the section of another lens, I find these parts retain their usual dissimilarity of structure,—the *outer* being softer, and the *inner* acquiring, by progressive laminæ, a horny density. The capsule of the lens is exhibited with considerable beauty, and is uniformly separated from the *proper* membrane of the lens by the interposition of a transparent aqueous humour. When the lens had been removed, the posterior part of the capsule retained its proper form and texture, but the anterior became like transparent horn. The eyes were those of the calf.

Some fœtuses, at different periods of conception, have been put up; but, as they had been in spirits of turpentine before being given to me by a medical friend, they do not afford any legitimate inferences, although well preserved.

An aphrodita, and some other small specimens, have been put up; and, deducting something for a little inattention to neatness, they correspond, in general, with the preceding.

About two months ago, I removed a pendulous tumour from the labium pudendi, which, during the pregnancy of the lady, always became œdematous, and as large as the feet; but contracted about one half after parturition. It was of very loose cellular structure, and, after remaining about a week in the solution at saturation, it corrugated, and greatly diminished. When it had been in water for twenty-four hours, it re-assumed its first appearance, and I put it into a weak spirit.

Whether diminishing the strength of the brine would have succeeded, I did not attempt to ascertain.

I have had mere fleshy parts in a covered pan for some months, which continue unchanged.

It remains for me now to relate those particulars which have appeared necessary preliminaries to success.

The solution I keep by me at saturation, which contains about 28 per cent. of salt. Mr. London has obliged me with two glass floats, one of which rises to the surface at saturation, and indicate s. g. about 1.206; the other rises at 1.160. These afford considerable facility in ascertaining the strength. By keeping the solution at its greatest strength, it can be reduced to any suitable degree.

When I put up a preparation in this solution, I add some very clear water, in the proportion of half an ounce to a pint. This is done to guard against crystallization; for, although even at saturation no crystals can form without evaporation, by adopting the above precautionary measure, a greater length of time would be requisite before inconvenience could result, should the escape of water not be completely prevented.

It seems best not to macerate the substance so long in water as is usually done for spirit; but, after having been in water, frequently changed from one to three or four days, it should be placed in some common receptacle, containing solution for some days or weeks as may be most convenient. A glazed earthen pan with cover answers very well. The specimen, by retaining some of the blood, looks more natural; and, by an earlier removal from the water, future decomposition is more effectually suspended.

On being first put into the solution, animal matters float; but, in twenty-four hours or more, according to their nature, they sink. Where, however, they have not sufficient gravity, no deformity arises from their suspending small glass bodies. The texture of some animals is such, that they will never sink unassisted; and, whenever this happens, threads over the rim may be dispensed with.

One objection has been suggested by high professional authority, viz. that, *in three years*, the preparation will shrivel.—I must adopt Bacon's maxim, and say, *fiat experimentum.* At present, no approximation to this state is visible; and, if I have a correct idea of the chemical action of muriate of soda on animal matters when *perfectly* excluded from the atmosphere, such a contraction is highly improbable.

The usual method of closing with bladder and lead is ineligible, for, when the bladder becomes saturated, the salt in time penetrates the coverings of the rim, and crystallizes on the bottle; and, independently of this exudation, the lead would gradually be decomposed by the solution. I have tried several modifications of this plan, but all seem uncertain or troublesome.

I now adopt a piece of common window glass, cut to the size and shape of the bottle, and fixed either by resin or putty. Putty answers tolerably well where there are no threads to sustain the specimen, but, where there are, they are liable to slide in, or to act as siphons, till it dries. Resin succeeds in all cases so well, as to justify a decided preference. Having wiped the edge of the bottle dry, I spread on it some melted resin, softened by a few drops of oil, and lay on it the glass. By holding over it a spatula considerably heated, the cement liquifies, and the glass fits with the utmost accuracy. After smoothing the resin, I have covered it in some instances with black varnish, without further trouble; but, as an additional security, it is preferable to besmear the edge with some glue, and lay over the top a piece of moistened bladder.

Persons are accustomed to lift their specimens by the rim, and, if the glass have any angular projections, and were unprotected by bladder, it might occasionally be raised; it is proper, therefore, to keep it a little within the circumference of the *round.* The centre of the bladder may be removed, so as to leave the largest part of the top diaphanous.

The description of manual operations, in which there are several minute objects for attention, is apt to convey an improper idea of the trouble attending them; whatever may be inferred from *reading* the above, I am persuaded those who *practise* it, will find it more easy and neater than the method with half-putrid bladders and lead.

No inconvenience has resulted when the diameter of the glass did not exceed two or three inches; but, at four or five inches, it has sometimes cracked, after having been finished some weeks. Whether this has arisen from variation in the pressure, or bad quality of the glass from not being well annealed, I am not satisfied. In two instances, I have used the *eye* of the glass, having its convexity central. Each of these has a diameter of four inches, and has been up four or five months; one remains perfect, the other, soon after being fixed, cracked about half an inch at two opposite points, but continues in *statu quo*, and is as effectual as at first, nor would the cracks probably have been discovered had I not sought for them. The convex top, I think, improves the appearance of the bottle.

Where bubbles have yet appeared in the specimens, maceration had been continued longer than is advisable, and it is suspected decomposition had commenced before they were put into the solution.

As the expense of this salt is very trivial, I intended to have ascertained whether it might not be subservient to important purposes on a larger scale, but circumstances have hitherto prevented. My design was to have procured a small adult subject, to have divided it in such a manner that all the important parts should remain in their natural position, and so place them in a covered tub of the solution. If this succeed, a practitioner might always have in his possession a subject for anatomical research; for, even, should it harden, maceration in water for twelve or twenty-four hours would render it sufficiently soft for dissection, and it might easily be re-salted. The proper strength of the fluid would be kept up without inconvenience, and almost without expense. Should evaporation happen, the addition of water will prevent crystallization; and if, by frequent examination and maceration in water, the strength be reduced, only the addition of a proportionate quantity of this muriate of soda would be needful. I cannot conceive that any thing more can be requisite to ensure success, than carefully to keep the animal substance beneath the surface of the brine.

In the event of success, the plan may be of incalculable advantage to country practitioners; for, however valuable dry preparations may be to illustrate the vascular system, no correct knowledge can be gained of anatomical structure, but by observing all the parts in their natural and relative positions.

The conveyance of the salt to any part of the country would be effected with the utmost facility.

Some attention to the dissecting instruments will be necessary to keep them from being corroded.

Whether the common muriate of soda would avail, I cannot determine; but, as the solid salt is more effective in the preservation of meat, it is reasonable to infer its superiority for anatomical purposes. The operation by which it is distinguished in its preparation from the common salt deprives it totally of the muriate of magnesia, which, I am informed, promotes putrefaction.

It must be understood, that the plan is chiefly recommended for those to whom economy is important; at the same time, it seems applicable to some purposes for which the volatility of spirit renders it unfit.

It did not concur with my object, to sanction these experiments with the approbation of distinguished characters, yet, independently of the personal honour I feel in being permitted to introduce the name of Sir Joseph Banks, the permission derives importance, from the influence which he has long illustriously diffused by a most liberal cultivation of the arts and sciences.

At the request of Mr. London, and by a previous arrangement with Sir Joseph, I laid before, and left with him, the specimen No. 1, and that of the eye alluded to above, and waited on him a few days since to ascertain the result. He said they had been shewn to several eminent anatomists who resorted thither, and, in general, the plan was considered novel,—much approved,—and deemed highly worthy of an extended trial. The specimen of the eye, he said, was especially admired.

Great Prescott street, Feb. 1, 1817.

A Case of Amputation, with some Experiments and Observations on the securing of Arteries with minute silk Ligatures. By John Cross, Surgeon at Norwich, Author of "*Sketches of the Medical Schools of Paris;*" late Demonstrator of Anatomy in the University of Dublin, &c.

[From the London Medical Repository.]

ABOVE fifteen months have elapsed since Mr. Lawrence published an interesting paper in the Medico-Chirurgical

Transactions,[*] on "a New Method of Tying Arteries;" and although the proposal of so able and celebrated a writer must have been received with eagerness and speedily adopted in practice by many surgeons, no additional information upon the subject has since been offered to the public. In the absence of communication from better sources, therefore, I have found the motive for putting together the few trials I have made, and begging the Editors of the *Repository*, if they should think them worthy of such a distinction, to submit them to the consideration of its numerous readers.

The originality of Mr. Lawrence's suggestion, to use fine silk ligatures, and cut them off close to the knot, has been questioned upon insufficient grounds. He not only seems to have undertaken this plan without knowing what was doing or had been done by military surgeons on the continent, but to have had a different view in the practice he suggested.

The number of sick that were often crowded into the same hospital, and the excessive fatigue which the soldiers had previously undergone, contributed to render the hospital gangrene very prevalent and destructive towards the conclusion of the late war. The progress of this disease was nearly in proportion to the extent of the wounded surface exposed; and whilst small wounds sometimes escaped, large ones were almost sure to suffer. Hence the military surgeons, who were the first in France to adopt the English plan of uniting stumps by the first intention, derived an additional motive for recommending it. Immediate union after amputation, diminished the frequency of hospital gangrene, but did not altogether prevent it, the ligatures keeping up a suppurating surface, which communicated with the external air. To avoid this inconvenience, the ligatures were cut off close to the knot, and the stump accurately closed; and as often as complete union took place, the patient was secured from the contagious disease.[†]

Most new things originate in some suggestion from existing circumstances. Whether the military surgeons of France or England were the first who cut the ligatures short, I am unable to determine; but the former seem to have tried it to the greater extent. Those who have noticed it in their writings, say nothing of the kind of ligature employed, from which we may fairly conclude that they use those of the common size; and although they probably never dreamt that the ligatures thus buried would be absorbed, or remain quiet in the stump for life, they were justified in leaving them to their fate, with the hope of obviating one of the worst complication of wounds with which the military surgeon has to contend.

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[*] Vol. vi. p. 156.

[†] "Pour éviter cet inconvénient," says M. Delpech, "nous avons pris le parti de ne plus réserrer des bouts de ligature, et de couper les fils contre le nœud, afin de n'avoir plus d'interposition, et de pouvoir faire une réunion exacte et complète. Dès lors n'ayant plus de plaie extérieure, nous n'avons plus eu de pourriture à la suite des amputations."—*Memoire sur la Pourriture d'Hôpital*, par J. Delpech, p. 29.

The ligatures were afterwards cut short by many surgeons in the army, without reference to the occasions that first rendered the practice advisable. When, however, there was no contagious disease to be prevented, the advantages gained were so small as to be easily outweighed by the unpleasant consequences that sometimes followed; and an excellent modern writer on Military Surgery has given his testimony decidedly to this effect: when the short ligatures have been buried in a stump, "I have seen,"* he says, "some ill looking abscesses formed by them, and I suspect some disagreeable consequences will occasionally ensue, if this practice be continued."

It is so well known that a stump will unite over a ligature of the common size, that the teacher has to direct the young surgeon to be careful in bringing all the threads out of the wound, lest such an occurrence should take place, and lay the foundation of future mischief. I have seen suppuration and breaking out of a stump six months after amputation of the fore-arm, and a ligature an inch long discharged, which had been overlooked in the closing of the wound, and buried under the integuments; and the blunders of operators have, no doubt, given many others an opportunity of ascertaining the same fact.

The use of ligatures of the finest silk, cut off close to the knot, is not proposed by Mr. Lawrence to guard against any prevalent and destructive disease, but to obtain a more speedy and complete union of wounds after operations, and to diminish the chance of hæmorrhage, by avoiding the irritation which large ligatures excite; and the statement which he detailed of his experience, gave me so much confidence in the safety of the practice, that I was induced, without previously making any experiments on animals, to try it, soon after that gentleman's paper came to my knowledge, in the following case of amputation.

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* Guthrie *on Gunshot Wounds*, p. 94.

M. E., a young woman, with engaging features and small, well proportioned body, aged 23 years, was seized with violent pain of her right knee, two years before, when in bed, and was soon succeeded by a degree of swelling. The disease, from this beginning, went on progressively; the particulars of its history I do not know, nor would they, collected from report, be of much value. Fomentations, blisters, and mercurial ointment, had been used at different times, previous to the occurrence of suppuration. During the last four or five months, she had been under the care of a particular friend of mine, a most respectable surgeon, who had evacuated abscesses around the joint, and kept open issues on each side of it for many weeks.

I visited this patient on the 26th of January, 1816; her suffering and the state of the diseased joint were such as called for the operation I went to perform. Her easiest posture was on the back, with the thigh drawn up towards the abdomen, the affected knee being permanently bent to a very acute angle, and the slightest attempts to alter the angle of flexure causing excruciating pain. The issues had been healed; there were two sinuses discharging ill-conditioned matter. Above the patella, was a swelling, extending three inches up the forepart of the thigh, and bulging out on each side of the tendon of the rectus femoris muscle, which seemed to contain a fluid. The thigh was wasted so as to give the knee an appearance of being greatly enlarged. The patient's health had not suffered seriously, so as to make the performance of amputation of urgent necessity; but she was tired of long protracted suffering, and had firmly made up her mind to the only means which, in such a state of disease, could afford her permanent relief.

It is unnecessary to mention the particular steps of the operation. In making the circular incision of the integuments, I had some difficulty in bringing my hand underneath, in consequence of the acute angle which the leg made with the thigh; and this compelled me to cut higher than otherwise was absolutely requisite. All the parts cut through were in a healthy state, the incision being some way above the swelling on the forepart of the thigh. I had with me some of the smallest dentist's silk that I could procure, yet of such strength that I could hardly break it by pulling with my hands, on account of its cutting into my fingers; with this I tied the main artery, (pulled out with the forceps so as to be free from all surrounding parts,) and immediately cut the thread off close to the knot. The tourniquet was then loosened, and five or six smaller vessels tied, being taken up with the forceps and separated as much as possible from surrounding soft parts. Upon some of these I only put one, upon the rest two, of the three twists which composed the silk with which the main artery was tied. I found an inconvenience from having cut off the first thread close to the knot immediately after applying it; in looking for other vessels I did not know exactly where the ligature already applied was situated; and, had it been on a smaller vessel, there might have been danger of my pulling it off with the forceps in searching for other vessels, as the stump was very little disposed to bleed. I therefore consider it a useful caution not to cut off any of the threads close to the knots, until all the vessels requiring it are tied. I observed this rule with respect to the smaller ligature; when I had cut these off, I brought the skin accurately together by strips of plaster, leaving a small space between each strip, and put on a very light covering of linen. After I had applied one or two strips of plaster, I pressed out several bubbles of air from under the skin, which, if left, would have done much mischief, increasing suppuration, and preventing adhesion by keeping the surfaces of the wound from being in contact. It is of considerable importance to be thus careful in dislodging every particle of air in closing the stump; the air is most likely to lodge where the integuments are abundant.

On the morning after the operation, the stump was easy and cool, and the dressings scarcely soiled with the discharge. On account of her residence being at a great distance from Norwich, I did not see the patient again; and the first report I received from the surgeon who continued to attend her, was not very flattering, a considerable quantity of unhealthy matter having escaped on taking off the dressings on the fourth day, and vesications having formed on the face of the stump. The reply which I made to this report will give some notion of it:—"As you seem to despond of our patient, I must do so too; but I confess you have not furnished me with all the grounds of such an opinion, unless I consider her being " placid and willing to die" as bad signs; for people are rarely willing to die, until they think there is no longer any chance for their living. The bad signs must be drawn from the pulse, the spirits, and the appetite; and if, in these respects (about which you have told me very little) there be nothing very unfavourable, I cannot think that matter forming, and a stump not uniting immediately, are sufficient reasons for at once desponding. If the integuments of the stump be flabby and inactive, it should, perhaps, rather be kept warm, and action promoted. At any rate do not let the patient sink for want of the strength being supported by good broth, bark, and cordials if required; and, as matter must form, leave a free opening for the discharge of it."

On the eighth day after the operation, I again heard of the patient. Her health had been improved by bark and nourishing diet; and cold applications, aided by the fan and bellows, had been found necessary for conducting off heat from the stump. At the end of three weeks, there was only a quill-hole opening on the face of the stump, discharging matter; the rest of the stump being very neatly united.

Six weeks passed away before the stump was quite healed; and during the four or five succeeding months, small collections of matter were repeatedly formed, and discharged; the patient being, however, able to go about well, with the assistance of an artificial leg. In January last, twelve months after the operation, I learnt that the stump continued to be soundly healed. None of the ligatures left on the arteries were found in the discharge from the stump; but I do not regard this as an argument that they were not discharged, as the researches for them were slight, and the secretion of matter very abundant.

A short account of the appearances found on dissection of the amputated limb, will not, it is hoped, prolong this paper to a tedious length.

The external openings, which had formed at different times for the evacuation of matter communicated with large cavities on the inner and posterior parts of the knee, extending half way down the leg, above the fascia enveloping the muscles. The swelling above the patella, which was supposed to contain matter, consisted of a semi-transparent gelatinous mass, streaked with white opake lines, into which the synovial membrane at that part had been converted. This mass, in some parts two inches in thickness, formed convex prominences, on each side the tendon of the rectus muscle, which were covered thinly by the expanded vastus internus and vastus externus muscles. The synovial membrane on all sides was diseased, in some parts being firmer and less gelatinous than just mentioned; in other parts of a light brown appearance; in others ulcerated through, forming communications between the superficial abscesses first described, and the cavity of the joint. The semilunar cartilages and their ligaments were entirely gone; the corresponding surfaces of the tibia, femur, and patella, were denuded and rough. The parts of the articular surfaces of these bones, which were still covered with cartilage, had it very slightly adhering to them, and no synovial membrane upon them. There was no pus accumulated in the cavity of the joint, but its surfaces were bedewed with a brownish discharge, like what used to flow from the external openings. The head of the tibia was soft enough to be cut with the scalpel, but none of the bones were enlarged. The lateral and crucial ligaments were healthy. The fixt and bended position of the limb seemed to be owing to contraction of the flexor muscles, and not to the diseased structure preventing the motion of the joint; for, after the diseased mass was taken away, the joint still remained fixed until the flexor muscles were cut through. The diseased mass, as it has appeared to me, from this as well as other instances, limits flexion and extension, but never goes so far as to fix the joint completely. There was much fat around the joint.

This is rather a striking example of the disease described by Mr. Brodie, in the fourth and sixth volumes of the Medico-Chirurgical Transactions, under the title of a Morbid Change of Structure of the Synovial Membrane. Like other morbid structures, it tends, after having proceeded to a certain extent, to go into ulceration; and this process, I am inclined to believe, commences generally on the outer surface of the diseased mass, and proceeds inwards towards the cavity of the joint. The superficial abscesses, which form before the joint becomes exposed, come in aid of this opinion; and I have repeatedly seen ulceration on the external surface of this morbid structure, without its extending deep into it.*

The use of the minute silk ligatures requires dexterity, and is attended with difficulties that amount to strong objections in the hands of many who have to perform operations. The vessel ought to be drawn out clean from all the surrounding parts, which is not always very practicable; and it may even be necessary, when there is a rapid oozing of blood from any spot, to use the tenaculum, and take up some of the soft parts with the vessel; in which case, to cut the ends off close to the knot, with a view of leaving the ligature to be buried in the stump, would be objectionable, however small the ligature employed. The experimentalist, who feels more than a common interest in the result of his trials, may give a degree of attention that will obviate all lesser evils; but in estimating a practice that is proposed for general adoption, we must calculate upon no more than a moderate share of attention and dexterity in the operator. It is clear to me that the surgeon, who has not a microscopic eye, will be longer occupied in applying the minute than the common ligature; the patient will be kept longer on the table, and the stump exposed for a longer time. Convinced of these points, I was desirous of prosecuting some experiments which might contribute to ascertain the advantages of the practice in question; an undertaking in which there was no great presumption, as Mr. Lawrence had acknowledged that he had not gone far enough in his inquiries to make up his mind upon the subject.

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* I have seen only one instance of this disease in the elbow joint. Amputation was performed at an advanced period; the diseased structure projected like a fungus over the edges of the articular surfaces of the joint, which were ulcerated and rough; and superficial abscesses communicated with the cavity of the joint by ulcerated holes through the diseased mass.—I have no doubt that the *Gliedschwamm*, or *Fungus Articuli* of German writers, refers to this disease. Böttcher, following the account first given by Brambilla, describes the disease as "increasing slowly, free from pain at the commencement, elastic to partial pressure like a sponge; and when there is much deep-seated pain, it indicates that the disease is complicated with suppuration and caries." The disease was considered to originate in the appendages and cellular substance around the joint, the anatomy and structure of the synovial membrane being at that time little understood. The morbid growth is certainly not long confined to the synovial membrane, but involves the neighbouring parts and converts them to its own nature; for after injecting a limb which had been removed on account of this disease, I have found the superior articular arteries, passing through the midst of the morbid and gelatinous mass, to reach their usual distribution.—(See *Böttcher, Krankheiten der Knochen, Knorpel,* and *Sehnen.* Band, 3, S. 225.)

Experiment 1st.—I exposed the right[*] carotid artery of a large rough coated water dog, and tied it with very fine Indian silk, which I could just pull tight enough, without danger of breaking it, to compress the artery. At the expiration of a fortnight, I put a single ligature of the same kind on the left carotid. The wounds from both operations healed quickly. The dog continued healthy, and became fat. Seven weeks from the first operation he was killed and injected. The injection was continuous throughout the whole of the right carotid, but the vessel was contracted at one part to half its former diameter, the ligature having only been drawn tight enough to compress, without obliterating the vessel. I found the ligature surrounding this contracted part of the vessel, buried in, and every where in close contact with a substance more dense than the arterial coats; I only detected it by very careful dissection, and the help of a magnifying glass. The knot was perfect, and the whole circle of the ligature entire. The left carotid presented very different appearances; two inches of it were obliterated, and converted into a dense cord, which was swollen out a little at one part to the size of a coriander seed; and here I found the ligature, surrounded by a firm and almost ligamentous substance.

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[*] I preferred the carotid, because it is situated so much deeper than the arteries of the extremities. The coats of the vessel were dissected clean before the ligature was applied, and the threads were always cut off close to the knot. In the instances in which dentist's silk was used, it had been previously washed.

From their situation, I am inclined to believe that these ligatures would have remained during the life of the animal. The history of extraneous bodies in the living system affords nothing in favour of a contrary supposition. Extraneous substances, after lying a long time buried and concealed, have excited suppuration, and made their way out of the body; but they have been weighty; and on this or some other account, have gradually changed their situation, until, coming near the surface, they have excited irritation, and given rise to a process which has enabled them to be discharged. Supposing these ligatures would cause suppuration, and be discharged externally, when brought near the surface of the body, their weight is not sufficient to change their place, and bring them into such a situation; nor is there any other process by which they are likely, as far as I can conjecture, to be transferred near to the surface of the body, when once surrounded by a firm ligamentous structure.

It is necessary, however, to consider the difference between applying a ligature to the continued trunk of an artery, and to the extremities of vessels exposed by operations. In the latter case, a portion of the vessel becomes nearly insulated, and the sloughing of this may involve the fate of the ligature. In subsequent experiments, I endeavoured to profit by this observation.

Experiment 2d.—I tied the right carotid artery of a thin small dog with one of the three threads which compose the smallest dentist's silk. I put on two ligatures, at the distance of half an inch from each other, and divided the vessel between them. In forty-eight hours the wound was soundly united, and I therefore took out the three stitches with which the wound had been brought together, lest, by remaining, they should cause the wound to burst open again.* The left carotid was tied, three weeks afterwards, with a single ligature of the same size; the animal tore the wound open, and it was not united until ten or twelve days. Six weeks from the first operation, the animal was killed and injected. The two ends left by the division of the right carotid were an inch distant from each other. Near the upper end of the vessel, there was a swelling bigger than a pea, which proved to be a cavity filled with matter; and, on puncturing it, the matter escaped, and with it one of the ligatures. I did not find the ligature which had been applied to the other end of this vessel. The left carotid was impervious for the fourth of an inch. The parts were all sound, and no ligature to be detected; it had probably escaped by the external wound, which was so long before it healed.

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* This remark is equally applicable to the human subject. Whenever sutures are used, they loosen themselves by the second or third day, and can do no good by remaining longer, but very often do harm, by causing inflammation, and re-opening of the wound.

Experiment 3d.—I put a single ligature of the smallest dentist's silk on the left carotid of an ass. On the same day, my friend, Mr. Stevenson, put two similar ligatures on the right carotid, and divided the vessel between them. The wounds suppurated, and much matter was discharged, in which the slough of one end of the right carotid was observed. On the sixteenth day from the operation, this animal was attacked with tetanus, and died on the twenty-sixth. Neither of the wounds had completely healed. On dissection, I found the three ligatures under different circumstances. On the right side, there was a purulent cavity down to the ends of the divided vessel, which were an inch and a half asunder, and both ligatures were found in the matter contained here, wholly unconnected with the artery. There was also the slough of one end of the divided artery, imbedded in matter, and unconnected with the ligature. On the left side, there was a cavity of pus at the part where the vessel was tied, and the ligature was firm upon the artery, not however covered over with any healthy living structure, but in contact with matter.

Experiment 4th.—A dog was kept four months after the application of very minute silk ligatures to each carotid artery. Both arteries were obliterated, the parts all sound, but no ligatures could be detected by the most careful examination. The probable explanation is, that the ligatures had escaped unobserved before the wounds closed, as these did not unite by the first intention. The results of other experiments render it quite unlikely that they were absorbed.

Experiment 5th.—Two ligatures of small Dutch twine were put on the left, and one on the right carotid of a dog, the ends being cut off close to the knot. On dissection, seven weeks afterwards, I found each of the ligatures in a small bed of matter, of the size of a pea, near the vessel to which it had been applied.

Experiment 6th.—I tied the right carotid of a young dog firmly with one of the three threads which compose the smallest dentist's silk. Three weeks afterwards, I tied the left carotid with two ligatures of the same size, and divided the vessel between them. Both wounds healed quickly and remained healed. The dog was killed at the end of thirty weeks, and successfully injected. On the right side, I found three quarters of an inch of the artery obliterated, and the ligature unconnected with it, being buried in a small quantity of brownish matter. On the left side I met with a similar abscess, of the size of a pea; and in the matter which it contained I found both the ligatures which had been applied to the artery precisely twenty-seven weeks before. The ends of the divided vessel had contracted so as to be above an inch and a half apart; hence it seems that these ligatures must, at one time, have been contained in separate abscesses, which had approximated until they coalesced.*

On both sides, I found a small vessel or two proceeding from the lower to the upper portion of the artery, as if nature had begun the work of forming a substitute for the part obliterated, in the way described by Dr. Parry. This is the only instance in which I have seen this process taking place, probably because I have not examined the parts at a sufficiently remote period from the obstruction of the circulation by ligature. These communicating branches were not above one-fortieth of an inch in diameter. Their enlargement seems to be a very slow and gradual process; but there is no reason why it should not go on after having once commenced, until the motive for this enlargement no longer exists, the blood taking the most direct course to the parts to be supplied with it.

The experiments which I have detailed, have, in some respects, afforded different results from those which Mr. Lawrence made, and lead to the following conclusions, which are not very favourable to the employment of the minute silk ligatures after operations.

1st. If the wounds do not unite by the first intention, the ligatures may escape with the discharge, without any inconvenience.

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* This may help to explain why I failed to find one of the ligatures applied to the right carotid in Experiment 2d, where the wound healed too quickly for it to have escaped.

2d. If common ligatures of twine are cut short, the wound may unite over them, and they may be found in abscesses after an interval of many weeks.

3d. If the finest dentist's silk be employed in the same way, the wound uniting over it, the ligature may be detached from the vessel, and remain buried in an abscess, where it will be found at different periods, from one to seven months; and this may happen, whether the vessel be firmly compressed with a single ligature, or divided between two ligatures, so as to imitate the circumstances under which vessels are tied after operations.

4th. If Indian silk, fine as a hair, be put round a vessel, so as to diminish its diameter, or to effect its obliteration, by just compressing its sides together, it may remain in this situation without exciting abscess, or producing any inconvenience. The ligature may be thus applied to compress an artery for the cure of aneurism; but not to secure vessels divided by operations. If a thin ligature be drawn sufficiently tight upon a vessel on the face of a stump to be secure, I am persuaded that the extremity of the vessel, which becomes insulated, as it were, must die. How often do we see this slough of the main artery of the thigh come away with the ligature!

Mr. Guthrie considers that cutting the common ligature close to the knot is a valuable improvement in all cases, that will not unite by the first intention, which is following the practice with a view diametrically opposite to that of its advocates; and I should almost have been tempted to say as much of the minute silk ligatures, had they not been recommended by a high authority. To heal a wound so quickly that these ligatures become buried, seems, however, no desirable object; since they frequently, if not always, excite abscess when applied to the deeply seated carotid of an animal possessed of a system much more capable than the human of resisting violence and repairing injury. The minute silk ligatures seem to secure well the vessels to which they are applied; and so do common ligatures of silk or twine; hæmorrhage from the vessels which have been tied being a rare occurrence in a healthy wound. The minute ligatures, when firmly tied, separate quickly, and pass away with the discharge; the common ligature, tied tight, also separates quickly, and might be removed much sooner than generally happens, were it not for the unfounded fears of the surgeon. The end of a ligature sometimes rises half an inch further out of the wound than at the preceding dressing, but resistance is felt because the granulations have grown so as to form an isthmus too narrow for the knot to pass. Where this change has taken place, no danger is to be apprehended from bringing the ligature away, as it is already at a distance from the artery to which it had been applied.

Ought the surgeon readily to give up the comfort of knowing that all the ligatures, particularly the one from the main artery, are away? Have the minute short ligatures any other advantages than that of avoiding the irritation of threads hanging out of the wound? Are there not sufficient objections to their being employed generally in practice, however applicable to particular cases? May not the surgeon, who is less frequently engaged in operating, include with the artery a neighbouring nerve?

I conclude with these queries, under the hope that those surgeons, who have been captivated with Mr. Lawrence's ingenious proposal, may answer them by giving publicity to their experience upon the subject.

REVIEW.

A Sketch of the Botany of South Carolina and Georgia. By Stephen Elliott. *Charleston, S. C. J. Hoff,* 1816—17. *Vol. I. Nos.* 1, 2, and 3.

WE have received the three first numbers of this work, modestly denominated a Sketch of the Botany of South Carolina and Georgia, but which, if we mistake not, will prove one of the most thorough and elaborate works yet published on the Botany of North America. The extent of latitudes, and variety of country embraced by the United States, renders them capable of supporting vegetables of the most numerous and diversified habits. Many plants of the torrid zone flourish in the long summers of Orleans and Georgia, while natives of Siberia and Labrador are found upon the mountains of New England. Our soil naturally divides itself into a number of regions, exhibiting very distinct Floras, and characterized by strong peculiarities in their vegetation. A description of the vegetables, contained in any one of those, is interesting from its local character, while, at the same time, it affords sufficient employment for the eye and pen of any individual. The labours of botanists may be much more beneficially employed, in faithfully delineating the plants of a limited district, than in superficially passing over the whole productions of a widely extended continent. The efficacy of this plan may be seen in Europe, where every country has its particular Flora, though many of them are not larger than our individual states. The consequence is, that their plants are minutely and accurately described, and far less subject to be mistaken than ours.

We have had two successive Floras of North America, although no one has thought of embodying the plants of Europe, Asia, or Africa. Michaux and Pursh have rapidly surveyed our continent, and made their reports of a vast number of species; yet many of these are accompanied with such short and hasty descriptions, that the inquirer is occasionally left in the same doubt, as if they had not written. It will not be until works like that of Mr. Elliott have appeared in the different sections of our country, that botany can be considered as having advanced toward perfection among us.

This work is rich in the number of its plants, and very satisfactory in their descriptions. It comprises, as is usual, the generic and specific characters, which are given both in Latin and English. After this follows a particular and full length description of the plant by the author, evidently made with much care and attention. The *habitats* and times of flowering are added, with occasional remarks on the properties of curious and useful species. The observations, on the medicinal properties of various plants, are made by Dr. Macbride, a gentleman to whose botanical and medical skill we are happy to bear testimony.

Mr. Elliott's work is peculiarly rich in grasses. Upon this difficult and interesting tribe, he has laboured with great success, and has occupied the principal part of his first number with delineations of his numerous species. The three numbers now published, extend to the middle of the class pentandria. The completion of the work will form an important acquisition to science, and will, we doubt not, be duly appreciated by botanists, especially in that part of the union which it is intended to illustrate. As a specimen of the manner of the book, we extract an article on the Ipomœa macrorhiza of Michaux, which has been supposed by Pursh to be the convolvulus jalapa of Linnæus. Mr. Elliott calls it convolvulus macrorhizus:

"C. foliis cordatis, simplicibus lobatisque, rugosissimis, subtus pubescentibus; pedunculis 1—5 floris. E.	Leaves cordate, simple and lobed, much wrinkled, pubescent underneath; peduncles 1—5 flowered.

"Ipomœa macrorhiza, Mich. 1. p. 141.

"C. Jalapa? Sp. pl. 1. p. 860.

"Ipomœa Jalapa, Pursh, 1. p. 146.

"*Root* perennial, somewhat fusiform, very large, weighing when old from 40 to 50 pounds, white, farinaceous, insipid. *Stem* twining around shrubs and fences, slightly angled, pubescent. The young *leaves* acute, and tomentose on the under surface, the old leaves frequently obtuse, and slightly muricate; petioles 1—2 inches long. *Peduncles* about 3 inches long, 1—5 flowered. *Calyx* pubescent. *Corolla* large; border obscurely 10 lobed, externally pubescent, white, tinged with purple. *Filaments* unequal, as long as the tube of the corolla, villous and purple at base. *Style* as long as the stamens, 2 cleft at the summit. *Stigmas* globose. *Capsule* 9—3 valved, 2—3 celled. *Seeds* dark brown, clothed with long silky brown hairs. The whole plant lactescent.

"Grows in the sandy soils of the Islands of Georgia and Carolina.

"Flowers June—October.

"This has been generally considered by modern botanists as the C. Jalapa of Linnæus; but while the external characters of the two plants appear in many respects to agree, the medical qualities of this by no means resemble those of the officinal Jalap. We must therefore conclude either that modern botanists have mistaken the plant of Linnæus, or that climate has totally changed or destroyed its active properties; or, what is equally probable, that the plant producing the officinal Jalap has been concealed by the jealous vigilance of the Indigenes from the curiosity of Europeans.

"To Dr. Baldwin I am indebted for the following observations on this plant.

"'My information respecting the medical qualities of the I. macrorhiza, Mich. has been derived from actual experiment. Six drams of the pulverized root has been given under my notice, without producing any cathartic effect. I have also subjected the dried root to the test of chemical analysis, and found it to contain no resin (in which the active powers of the officinal Jalap reside) or so small a quantity as not to prevent its being used as an article of diet. It contains a great deal of saccharine, along with a considerable quantity of farinaceous matter. It is probably not more cathartic than the C. Batatas, nor contains more resin. Negroes I have been informed sometimes eat it.'"

Observations on the nature and cure of Dropsies, and particularly on the presence of the coagulable part of the Blood in Dropsical Urine; to which is added, an Appendix, containing several cases of Angina Pectoris, with dissections, &c. By John Blackall, m. d. *Physician to the Devon and Exeter Hospital, and to the Lunatic Asylum, near Exeter.* 8 *vo. London,* 1813.

THE opinion that dropsy is to be considered rather as a symptom, than as a disease, would probably be assented to by every good pathologist of the present day, and could be supported by the authority of many of our predecessors. It is, however, an important symptom; it gives a character to the disease in which it occurs; and a minute investigation of its causes, its varieties, and of all the morbid changes connected with it, as well as of the treatment adopted for its removal in every case, demands our grave attention. Some general truths on this subject seem to have been well ascertained.—They have opened the way to a more full understanding of it, but they have not hitherto led to any very important practical results; or at least not to such as are sufficiently precise. The belief that dropsy is a certain evidence of debility is very generally done away; it is attributed in many cases at least to inflammation, and not a few practitioners have shown that, in some instances, it calls for blood-letting.

That the effusion of a watery fluid in a circumscribed cavity is to be considered a process of inflammation *in all cases*, is not yet, perhaps, very generally believed. We are prepared however, to support this general explanation of dropsy, and as this is going somewhat further than even our author does in explicit terms, it is proper first to explain our views, although it must be done briefly. This will lead the way to an exposition of the valuable contents of Dr. Blackall's book.

In the case of hernia humoralis, the dropsical effusion is clearly referrible to a low inflammation, produced in many instances by a slight contusion. It is to be remarked that in this case, the inflammation is not accompanied by weakness; nor can we see any reason to believe that the affection is of a specific kind, such as to occasion the aqueous secretion instead of suppuration. The inflammation is only low; its processes are conducted with sufficient strength, but there is not the same intensity of disease as occurs where suppuration is to be effected.

In some other dropsical affections it is equally evident that inflammation exists. Inflammation in the brain produces *hydrocephalus internus* among children in the most unequivocal manner. So also hydrothorax follows pleuritis in some cases, and perhaps more frequently ascites follows peritonitis. In respect to anasarca this is sometimes occasioned by an inflammatory affection of considerable extent in the cellular membrane. But perhaps more commonly the inflammation in this membrane is of small extent, and the serous fluid poured out from the inflamed part is diffused over the whole body. Some cases in the book before us seem to explain this matter very well.

But it will be asked, how the explanation, which has been offered, will apply to those dropsies, which arise from organic diseases interrupting the circulation, and to those œdematous swellings which we attribute to debility. First, of the cases of interrupted circulation. Here the capillary vessels suffer undue distention, and an embarrassment ensues in the functions of all those extreme vessels, by which nutrition and secretion are performed. That a continuance of this distention and embarrassment should produce inflammation can be very easily conceived. That they do produce that effect we have distinct evidence on various occasions. In cases of organic diseases of the heart, pulmonary catarrh is often produced very suddenly and with great violence. It sometimes subsides again with equal suddenness; owing to the immediate relief from distention in the small vessels, which ensues when the heart is relieved for the time. Here is inflammation of the mucous membrane of the lungs, and we may discover the existence of this in various degrees. Sometimes it is strongly marked, and in the course of a few hours there takes place a great expectoration of mucus, accompanied by other symptoms of inflammation in the mucous membrane of the air passages. Sometimes this expectoration is quite watery, the matter of it differing very little from that which, being poured out from the pleura, would create a dropsy. Sometimes blood also is expectorated, as it is in the forming stage of peripneumony.

Accordingly, the same occurrences take place in the pleura, in organic diseases of the heart, with only a little modification arising from a difference of texture and properties in that membrane. Indeed the difference in these respects produces less variation in the circumstances referred to, than in common inflammation. Commonly in chronic affections of the heart the pleura pours out serous fluid; but occasionally coagulable lymph is effused, and sometimes pus is formed. Commonly the affection of the pleura is not such as to occasion acute pain; although frequent stitches and other uneasy sensations occur in it. But in some instances there is the acute pain of severe pleurisy, and even in a most violent degree. This is more immediately relieved by blood-letting than in common pleurisy; and that happens, no doubt, because, in the first place, the embarrassment in the functions of the heart is instantly relieved; and in the second place the distention of the capillaries is taken off by the bleeding. It is to be added to what has been said, that from the pleura, as well as from the mucous membrane of the lungs, blood is occasionally poured out in consequence of the interrupted circulation.

It would be easy to show that analogous effects take place in other parts, when the heart is diseased. Not only the ascites and anasarca admit of a similar explanation; but affections of the brain, of the substance of the lungs, i. e. of their cellular membrane, of the liver, &c. are to be accounted for on the same principles.

Secondly. How can the occurrence of inflammation be considered the cause of watery effusions in cases of debility? That such effusions in the lower extremities, and sometimes in other parts, occur on many occasions, which have nothing in common among them except debility, is well known. If in other cases such effusions are produced by a low inflammation, it is to be presumed that the same cause operates in these also; and we should therefore be ready to admit any doctrine which explains the production of that cause. Now it should be remarked, that it is not in cases of muscular debility, but in those in which the circulation is performed in a very languid manner, that the œdema most commonly takes place. In these cases it is obvious that the inability of the veins to carry the blood to the heart, and especially in an erect position, will produce the same effects on the small vessels from which those veins lead, as if the circulation were impeded by an organic disease of the heart, or by any other mechanical cause. The distention of the capillaries is the immediate exciting cause of the inflammation. This disease exists in the extreme vessels, not ordinarily carrying red blood; in those vessels, which are principally instrumental in every case of inflammation, as they are in secretion and nutrition.

It will surely be admitted, as somewhat in favour of this theory, that it leads to one uniform explanation of the occurrence of dropsy. It may be shown that the various methods of cure, which are sanctioned by experience, admit an explanation in accordance with the theory above stated. In one class of cases the remedies are such, as are employed for the removal of inflammation in other diseases. These are frequently successful, if the inflammation be merely membranous; but not often permanently so, if the inflammation has produced considerable organic injury, or change of structure. In another class of cases, where the inflammation is produced and maintained by obstructions to the circulation, the remedies must be such as obviate or remove those obstructions, whether these arise from mechanical obstacles, or from debility. In all cases relief is most frequently accompanied by an increased flow of urine. Hence it is that various articles have acquired the reputation of diuretics, which do not manifest that character at all, except in cases of dropsy. These articles operate, as we believe, in removing inflammation, or in otherwise curing the disease;

and then the effused liquid being absorbed is carried off by the kidnies.

But a very slight examination of this theory will show that many important points remain to be settled. The aqueous effusion is referred to inflammation; but it is still to be shown in what respects this dropsical inflammation differs from the adhesive and the suppurative, whether it is specifically different, or whether it differs only in degree. It is to be shown wherein consists the difference in the various cases of dropsy, which we observe, in what modes they affect the general system, or any of its parts; and what are the methods of cure adapted to each variety of the disease. It cannot be said that Dr. Blackall's book has given us full satisfaction on any of these points; but it has nevertheless done much to help us. It has opened important new views, has pointed out some very useful distinctions, and has done something toward discriminating the cases, to which the different remedies in use are severally applicable.

The most prominent circumstance noticed by Dr. Blackall in respect to dropsy, and that which led him to turn his attention to the subject, is this, that in certain cases of that disease the urine is found to contain albumen. This circumstance had not altogether escaped the observation of preceding writers, several of whom are mentioned in the preface to this work. It had been particularly noticed by Dr. W. C. Wells, whose observations will be distinctly stated presently. But excepting Dr. W. the subject does not seem to have arrested the full attention of any physician before Dr. Blackall. This gentleman having found that albumen exists in the urine of many dropsical patients, but not in that of all such, has made it his business to notice what other circumstances are peculiar to cases, which differ from each other in this respect. The method which he has adopted is to relate a large number of cases in detail, and thus to present to his readers a store of facts from which his own opinions have been formed. Afterwards he has given his own observations on the various appearances, and has discussed the causes of hydropic effusions, bringing into view other diseases, or rather other forms of disease, which he believes to be essentially allied to dropsy. Lastly, he has treated of the cure. A postscript and an appendix close the volume.

In stating his cases Dr. B. treats "*first*, of those dropsies, in which the urine is not coagulable by heat; and *second*, of those in which it is so coagulable in a greater or less degree." In cases of the first kind there are various appearances of the urine, and these are considered.

In the second chapter we have seven cases "in which the urine, not coagulable by heat, was pale, crude, and without sediment, sometimes copious." Of these cases three were fatal; but we have to lament, that the subjects of them were not examined after death. These seven cases seem to have nothing common in their character, except the similarity in their urine, nor can any inferences, unless they be negative ones, be drawn from them. For instance, two of the cases originated in scarlatina, although it will appear afterwards, that in almost all cases from the same origin the urine is found to contain albumen.

Chapter third contains "cases of ascites and hydrocephalus, in which the urine was rather scanty, but in other respects appeared to differ little, or not at all, from the healthy state." There are three of these cases. The two first are ascites with anasarca, both following diarrhœa; and this in one of them was occasioned by a mercurial course. The third is a case of hydrocephalus, attributed by the author, and with much probability to the too liberal use of mercurials. On examination after death, there were found within the cranium evidences of inflammation and watery effusions.

Chapter fourth has cases "in which the urine not coagulable by heat, is scanty and high-coloured, becomes extremely turbid on cooling, and deposits a copious sediment, more or less lateritious." These appearances are considered by Mr. Cruikshank as evidences of diseased viscera. That, however, these appearances do not always occur, where the viscera are diseased, is manifest from some cases given in the second chapter of this work. Yet the frequent occurrence of these appearances in cases of organic disease, both with and without dropsy, can scarcely have escaped the observation of any practitioner. It may be noted also, that these appearances are not present at all times in organic diseases, even of most important viscera; but in many instances at least, only during periods of particular derangement, or irritation in the diseased organ; the derangement or irritation arising from inflammation. Thus, in organic diseases of the heart, the urine is perfectly natural at many times, but is such as is above-described during certain paroxysms of increased irritation in that organ.

The contents of this chapter are arranged in three sections. In the first of these are found cases of dropsy from diseases of the liver and of other viscera, several of them preceded by intermittent fever. In some of these cases mercurials gave relief; in two of them the same medicines did not relieve, but occasioned the urine to become coagulable by heat. The tenth case in this section deserves mention, as showing how different organic diseases resemble each other, although not as throwing light on the principal object of research. It was a case, of which the symptoms appear to give certain evidence of organic disease of the heart. On dissection there were found some slight marks of inflammation on the pericardium, and on the peritoneal coat of the liver, and stronger marks of inflammation on the pleura. But the most important thing discovered was "a scirrhous mass of the size of a large man's fist," situated behind the trachea at its bifurcation. "It extended from that spot a considerable way into the substance of the right lung, principally surrounding the bronchial tube." "In two or three parts of it were more defined round soft masses, resembling lymphatic glands beginning to suppurate." The great embarrassment and distress, attending the functions of the heart and lungs, are undoubtedly to be attributed to this tumour.

Were we not instructed by observation to speak with modesty on these subjects, we should attribute to organic disease of the heart more of the subsequent cases in this section and in other parts of the work than our author does. Even the 21st case we should be disposed to consider of this kind, so far as the brief statement of it would enable us to judge, if the author had not given us the dissection, in which no mention is made of morbid appearances about the heart; unless indeed we consider as such "somewhat more than the natural quantity of fluid in the pericardium." In this case, with universal anasarca, we find the pulse 110, weak, irregular, intermittent; dypnœa, nightly suffocations; loose bowels; urine brown, scanty, foul, with brauny sediment, not coagulating by heat. On examination, there was water in the thorax and abdomen as well as in the cellular membrane; but the only organic change was in the liver. This organ was "much harder and somewhat smaller than natural, and the lower edge slightly curled and bent forwards. Its substance, when divided, exhibited no very peculiar structure, but an unusual degree of firmness and solidity. This Dr. Baillie considers as the first step towards the tuberculated liver, more commonly called scirrhous."

In the second section of the fourth chapter are contained "general remarks on the preceding cases, and particularly on the dropsy that attends the intermittent and Walcheren fevers." These remarks are worthy a careful perusal. Several circumstances are pointed out respecting urine not containing albumen, but yet having an unusual portion of other animal matter. In respect to the dropsies from visceral disease following intermittents, and particularly the Walcheren remittent, it is observed, that mercury often fails and even is sometimes injurious.

The third section of the same chapter is "on hydrothorax, its symptoms, causes, and cure by squills, calomel, &c." The symptoms, their progress and changes, are very well detailed; but there is no attempt to distinguish between simple dropsy of the chest, and the complicated disease, in which that dropsy originates from unsound viscera. In that respect the author resembles Dr. M'Lean, who in his very valuable work on hydrothorax fails in respect to the same discrimination. Corvisart attempts to make it, and is very dogmatic on the subject; but we have learnt that upon this point he could not be relied on. We do not despair of seeing the diagnosis well settled at a future day, since observations on the living and on the dead are daily made with more and more accuracy. Dr. Blackall refers most cases of hydrothorax to organic diseases, some however to original inflammation of the pleura, and this occasionally to the influence of gout. But of the organic diseases he appears to consider the greater part as existing in the liver. We believe that he does not allow enough to the heart. Perhaps the truth may lie between his estimate and that of Corvisart. Dr. Blackall remarks with surprise, that water is accumulated in the chest earlier than in the abdomen, in many cases of diseased liver, and acknowledges that "it does not so readily appear why this should ever take place." If the heart be diseased in many cases, in which he charges the fault to the liver, the difficulty in the explanation will be removed in great part. It must be confessed however, that in one case mentioned above, and perhaps in some others, his opinions find much solid support.

As to the cure of hydrothorax, of the kind now under consideration, the author thinks that in the early stage much may be done; principally by diuretics; of which he considers *squill* by far the most powerful. To avail ourselves of the virtues of this remedy, a minute attention must be given to its dose. It must be given in the fullest quantity, which the patient can bear without sickness. The benefit may be somewhat increased by nausea; but this cannot be supported long; and it tends to give the patient such a disgust to the medicine, that afterwards the entire suspension of it sometimes becomes necessary. The form of administering this drug is not very important; but its liquid preparations, such as the vinegar or tincture of it, are rather preferable. The *mistura ammoniaci* and *spiritus ætheris nitrici* seem to assist its operation. The evil effect of the squill on the digestive organs is to be guarded against. If these organs have their functions much deranged by its use, it must be relinquished. Such are some of the remarks of

Dr. Blackall on this subject. He considers the *colchicum* as "probably adapted to the same occasions as the squill. But he has not had much experience of it."

"When the squill does not act entirely as could be wished, the addition of a grain or two of the submuriate of quicksilver, every night, is frequently followed by a great flow of urine, at the same time that the salivary glands are affected." This medicine does no doubt give effectual relief in some cases of organic disease, on which dropsy in the chest, and in other cavities, depends. For hepatic obstructions it has been too much relied upon as a specific. In the liver, as in other viscera, a common chronic inflammation may be often removed by mercurials. But on the scirrhous, or tuberculated state of that gland, Dr. Blackall has seldom seen them produce any impression. Our own experience fully accords with his in this particular, and we think the observation important; for, in common practice, mercury is resorted to and persevered in for many such cases, not only without benefit, but with great injury.

Dr. Blackall has found squills so effectual in hydrothorax, of the kind here referred to, that he has not made much trial of digitalis. On this subject we may refer our readers to two most valuable works in which the subject is fully considered; the first by Dr. Wm. Hamilton on digitalis, the second by Dr. M'Lean on hydrothorax. Our own experience has afforded many opportunities of witnessing the good effects of this remedy in hydrothorax, more indeed than in any other dropsy. The cases, in which it has been useful, have been some of them certainly referable to organic diseases of the heart and large vessels; others have been supposed to arise from other causes. In the cases arising from scarlatina, in which the urine is coagulable, it will be seen that Dr. Blackall approves this remedy

Dr. Blackall objects both to purgatives and tonics, and makes no mention of vesication. This last remedy ought not to be omitted; since it often affords a temporary relief under very distressing circumstances of the disease in question, and seems even to arrest the progress to the grave, while other remedies are operating a more slow, although a more effectual relief.

Chap. 5th is "on dropsies, in which the urine is coagulable by heat." The presence of albumen in the urine is not entirely confined to dropsies. Dr. Blackall suspects this occurrence in cases of great emaciation, not accompanied by any unusual discharges, and not otherwise easily accounted for, and he detected it in a slight degree in two instances. This chapter has some useful remarks on a kind of anasarca resembling land-scurvy.

Chap. 6th, "on the anasarca and general dropsy, in which the urine is coagulable by heat." One of the exciting causes of this form of the disease, mentioned by Dr. Blackall, is scarlatina; another is the imprudent use of mercury, perhaps aided by cold; a third the drinking of cold water, when heated; and under a fourth head he arranges "those cases, in which the exciting cause is not very obvious nor precise, but appears connected with different circumstances of fatigue, cold, the use of strong liquors, visceral disease, or the injudicious employment of tonics."

Section 1st contains cases of anasarca, &c. after scarlatina. The first was a bad case, diuretics were useless; scarification gave some relief; erysipelas of the lower extremities and gangrenous spots ensued; now the cinchona and port wine were given very freely, and not only the erysipelas, &c. were arrested, but the urine flowed in abundance and the patient perfectly recovered. The bark and steel had both been tried at an earlier stage with aggravation of the symptoms. In the next eight cases, digitalis, followed in some of them by cinchona, effected cures. In these, calomel was hurtful. In some of these cases, it was obvious that water had collected in the chest. In respect to the tenth case, it is doubtful, whether the preceding disease was scarlatina; we are only told that it was "a fever with great redness of the face and extremities." "Soon after this she became dropsical and was affected with stricture of the chest, cough, and pain under the left breast." The dropsy was relieved, but not the whole disease, and the patient died. On examination, the kidnies were soft and flaccid, and were considerably loaded with fat. In the chest it was found that the pleura had been severely inflamed, that adhesion had ensued, and in the centre of the adhering parts suppuration had taken place.

In five of these ten cases the urine contained blood. Dr. Blackall thinks this is promoted by the use of mercury; but it occurs also from other causes, and especially in dropsy after scarlatina.

In these cases of dropsy, following scarlatina, in which the urine is coagulable by heat, Dr. Blackall calls digitalis the sovereign remedy. The first cases in his book show, that the same medicine is not to be employed in cases of similar origin in which the urine does not coagulate by heat.

Section 2d contains cases of anasarca, &c. from mercury. Four very serious cases are given under this head, and deserve very careful consideration by those, who believe that mercury never does any harm. Yet the evidence is not such, as would authorize us to conclude definitively, that the author is entirely correct in his opinions on this point. The first case was relieved by bleeding, and ultimately cured under the use of bark and wine. The second was relieved by digitalis; but was ultimately fatal, no doubt in consequence of the improper use of this medicine by the patient. The third and fourth cases were fatal. In the last, examination after death shewed very extensive disease. The pericardium and pleura were greatly inflamed, the peritonæum about the liver and spleen more slightly. Marks of inflammation appeared also in the cellular membrane, and likewise other morbid appearances were discovered. The dissection is a very instructive one.

The two last of these four cases are thought by Dr. Blackall to be "specimens of a true mercurial habit, slowly forming, distinguished in their advanced stage by a most unconquerable buffiness of the blood, and an unusual colour even of the serum."

There are added in this section five more cases of slighter dropsical swellings, attributed by the author to the use of mercury. The fifth of these cases is interesting, as displaying a remarkable scrophulous affection of the intestines.

Section 3d has two cases of "anasarca, &c. from drinking cold water, when heated and fatigued." The only question in respect to these and other cases of disease, said to be induced by drinking cold water, is whether the thirst is not the first strongly marked symptom of disease, which is immediately afterwards more fully developed. If it be not so, we are at some loss to explain why this very common practice is not more frequently followed by injurious effects.

Section 4th has nine "cases of anasarca, &c. from cold, intemperance, &c." The four first cases terminated fatally. These and the cases in the preceding section afford evidence of great value in support of the opinion that dropsy is to be attributed to a process of inflammation.

Chapter 7 is upon *cachexy*, a depraved habit of body, and is worthy a diligent perusal by every one, who would comprehend the true causes of many of those cases of general debility and broken constitutions, which are so often met with. The relation of cachexy to dropsy is very well shown. Many other useful remarks are added, in relation to a depraved habit of body. It must be obvious, that this depraved habit arises proximately from a vice in the functions of the extreme vessels in every part. This may have its source in profuse evacuations, whether of blood or other fluids; in unsound viscera; in affections of the mind;—and perhaps in other causes. In the majority of cases a cachexy is marked from the first by some disorder of the abdomen, as Dr. Blackall states; but it is not peculiar to disorders of the organs in that cavity.

In this chapter we find five cases, in all of which the urine contained albumen, and of which the three first were fatal, and also the fifth. In the three first cases the liver was undoubtedly diseased, but the bodies were not examined. The fifth case was attended by a great variety of distressing symptoms, and, on dissection, there were found vestiges of disease in each of the great cavities. The case is instructive in several respects, which we cannot notice; but particularly in this, that it teaches us not to insist upon referring all the symptoms, in every case, to one single source. Dissections will often teach this lesson; and although we should not multiply causes unnecessarily, it deserves to be called to mind in difficult and obscure cases.

The fourth case terminated favourably, after four bleedings, and the use of digitalis and opium. The symptoms do not clearly point out the seat of the local disease; but we are disposed to believe, that it was in the chest. But the symptoms of debility and of cachexy were strongly marked, and such as would have deterred many systematic practitioners from the use of the lancet.

Chapter 8th contains valuable matter. It is on cases resembling land scurvy. The result is, that inflammation and organic diseases produce a variety of effects in the different systems of the body, which have been called by different names. The distinctions, in the appearances pointed out by the names, should not be overlooked. We should study to ascertain, by what derangement in the functions these different appearances are produced, and from what differences in the primary affections they respectively take their origin.

Chapter 9th contains "cases of hydrothorax, in which the urine was coagulable by heat." We have here additional evidence, that dropsy is a consequence of membranous inflammation. Dr. Blackall appears to think, that when the serous membranes continue to be inflamed for a long period, although they may be partially restored, they cannot easily be brought back to their original state. At best, they continue susceptible of disease, and every irregularity in diet and regimen will occasion a new access of inflammation. These remarks apply, mutatis mutandis, to most chronic diseases, after they have once become established, whether these depend on inflammation, or on spasm, or on peculiar changes of structure. In these last cases, the irregularities referred to, seem to induce sometimes a turgescence of vessels, and sometimes an actual inflammation around the part, in which the structure has become changed.

In the cases, described in this chapter, Dr. Blackall employed digitalis more frequently than in the cases of hydrothorax, in which the urine is not coagulable by heat.

Chapter 10 contains "cases of ascites, in which the urine was coagulable by heat." The same general inferences are to be made from this, as from the last and other chapters, in respect to the proximate cause of the disease. It contains evidence, deemed of great weight by the author, against the free use of mercury. He suspects both the liver and the kidneys to have been diseased from the use of that article.

Chapter 11th is on "hydrocephalus, in which the urine was coagulable by heat, or contained a bloody sediment." The first case originated in scarlatina; this occasioned anasarca, which, subsiding suddenly, was followed by well marked symptoms of hydrocephalus. Bleeding, blistering, and digitalis gave relief, and the cure was confirmed by cinchona. The second case is attributed to mercury. To the subject of it, aged seven, two scruples of calomel had been given within forty-eight hours. The disease was fatal.

We have now presented a view, as full as our limits will permit, of the most important facts stated in the work before us, and have occasionally suggested the principal inferences to be made from them. The remaining chapters deserve a very careful perusal. They are of great practical import. It is in vain, however, to attempt to give an analysis of them; for it would lead to error, if we were to give general remarks and directions without the careful and minute qualifications which accompany them. It is then only necessary to add, that the 12th, 13th, and 14th chapters, contain a review of the facts ascertained with a general discussion of the nature and causes of different hydropic affections; and that the 15th chapter is on the cure.

It would be unjust in us to omit to notice the postscript. On our first perusal of the main body of the work, we were greatly surprised that, while credit was given to other authors, who had observed the presence of albumen in the urine of dropsical patients, no mention was made of Dr. W. C. Wells, of St. Thomas's Hospital, London. In 1812, a year at least before Dr. Blackall's book was printed, Dr. Wells had given two papers relating to dropsy, in the third volume of Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge. These papers were written principally to bring into view the presence of coagulable matter, in the urine of dropsical persons. One of these papers is entitled "Observations on the Dropsy, which succeeds scarlet fever;" the other, "on the presence of the red matter and serum of the blood in the urine of dropsy, which has not originated in scarlet fever." The work here described did not fall into Dr. Blackall's hands, until long after his own book was in the press. He, therefore, felt bound to notice the papers by Dr. Wells distinctly in a postscript.

First, as to the dropsy from scarlatina, Dr. Wells gives more evidence than Dr. Blackall does, of the inflammatory nature of the affection. "In a large proportion of the cases, he found red blood to be deposited in the urine; in many more it was foul when made, and threw down a lymph or mucus; in all but two, and those slight cases, it was coagulable."

Next, as to dropsy from other causes, Dr. Wells found the urine to contain albumen in seventy-eight cases out of one hundred and thirty. In three of these cases, red blood was observed in the urine. The proportion of cases, in which the urine contained albumen, was not the same under all the varieties of the disease.

"Amongst twenty-nine cases of anasarca not symptomatic, were twenty-three with urine containing serum; in several to a considerable extent. Of nine cases of anasarca, preceded by some debilitating disease, as dysentery, ague, &c., in only two was the urine at all coagulable. Of thirty-seven cases of anasarca preceded by cough and dyspnœa, there was serum in the urine of twenty-four. Of twenty cases of hydrothorax, serum in the urine of fourteen, in only one to any considerable quantity. In four encysted dropsies, three attended by anasarca, the urine contained no serum. Of twenty-one cases of ascites not preceded by anasarca, there was a little serum in the urine of seven. In eight cases of ascites preceded by diffused dropsy, it contained a very large proportion."

In detecting the presence of albumen in the urine, Dr. Wells employed nitrous acid as well as heat. The nitrous acid will sometimes detect albumen, when heat will not. The failure of heat to produce the coagulation, he attributes partly "to a deficiency of salts in the urine." In order to determine the quantity of serum contained in urine, in which coagulation is produced, Dr. Wells instituted a number of experiments, from which he deduced the following results. If serum make one fourth part of the urine, the whole will form a very firm and solid coagulum; if one fifth the coagulum will barely be solid.

"The lesser quantities, as determined by a similar mode, appear to be very various, from 1-640th of the mass to 1-10th, 1-8th when the whole is converted into a semifluid jelly, 1-7th a firmer jelly and beginning to adhere to the phial, 1-6th a still firmer jelly, great part of which adheres to the phial. Agreeably to this estimate, he calculated that one patient passed four ounces of serum in the day, another five, another seven.

Dr. Wells found, that albumen exists in the urine of persons affected with other diseases besides dropsy.

"An examination was made in nineteen cases of acute disease. In fourteen of them the urine was altogether without serum, in four it had a very small quantity, in one about 1-160th.

"Of eighty-five chronic cases, in fifty-four the urine had no serum, in twenty-five a very little, in one somewhat more, but likewise mixed with pus from a disease in the kidneys; in four who had been using mercury freely, rather a larger quantity.

"On examining the urine in six patients before they began the use of mercury for the cure of syphilis, in five there was no serum, in the sixth a very small quantity. After a salivation continued for a fortnight, in the last mentioned the serum was much increased, in three others some was detected, and in two there was none. In one who became dropsical after a free use of mercury, the urine contained no serum; in three others a large quantity.

"From theſe observations it is concluded, that urine containing a considerable quantity of serum, very rarely, if at all, occurs in any disease in this country except dropsy, and even in those very rare cases, has been induced by mercury."

It appears from the above, that Dr. Wells had observed the same effects from mercury, which have been pointed out by Dr. Blackall. Both gentlemen remarked, that besides other morbid appearances, a diseased state of the kidneys may often be noticed in persons who have died from dropsy.

With respect to the cure, Dr. Wells decides in favour of blood-letting, and other similar measures, in the dropsy which follows scarlatina. In dropsy from other causes, in which the urine contains albumen, he was induced to make a trial of cantharides internally. The remedy was employed in five cases, and appeared to be beneficial in three of them.

A few words must be added upon the Appendix. This is a very useful paper, occupying upwards of fifty pages, upon *angina pectoris*. It contains five cases, of which four terminated fatally, and the dissections are given. The fifth case is remarkable from having been induced by external violence. Its subject was a coachman, who received a blow on his breast from the pole of his carriage. After the first effects had subsided, he continued to feel a difficulty of respiration on motion, and at the end of two months well marked symptoms of angina pectoris supervened. Under the direction of Dr. Blackall he used some very mild laxatives, practised great abstinence, avoided exertions of all kinds and kept a seton open on his chest. The author adds, that he "has been now for more than three years regaining a firm state of health, and has returned to his employments."

In respect to the appearances after death, the author gives an account of all the cases known to him, including those in the very learned and ingenious work of Dr. Parry on this subject. The number of these cases is twenty. In three of these there was no appearance of ossification of the great vessels.

"In all the remaining cases, there was some alteration of structure tending to ossification about the origin of the arterial system, in the greater part, very extensive. An unsoundness of the aorta in various degrees is the most frequent appearance, being found in fifteen of these seventeen cases. In nine the coronaries likewise were ascertained to be diseased; in one the coronaries alone;* in several the valves and heart itself.

"That when this process begins in the trunk of the aorta, it should spread to the branches, and particularly perhaps to the coronaries, was to have been expected. But dissection shews, that it has not always done so before the paroxysms have proved fatal."

This paper merits a perusal by those who would understand what is known on this remarkable disease. In respect to the whole work which has been under review, it should be read twice over by every man who treats the chronic diseases of his fellow men. The practical information it affords is by no means confined to dropsy, and many important hints will be overlooked in a single perusal. It is among the works which will bring into disgrace the dogmatic practice of classing all chronic diseases together and referring them to dyspepsia, to chronic weakness, or to morbid excitement. We indulge the hope that the author will continue his investigations, and that he will be aided in his labours by others possessed of equal acuteness, and learning and judgment. We also indulge the hope that the medical public in this country will give so much encouragement to useful medical publications, as that we may shortly see reprinted here this and many other modern English works of great value, of which at present the names are scarcely known among us. We take occasion to make this remark at this time, because there is an intention among our booksellers to reprint several valuable medical books, if proper encouragement be afforded them.

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* This case, where the coronaries alone were ossified, is the only one the symptoms of which are not accurately detailed, but said generally to be those of angina pectoris. See Parry on Syncope Anginosa, page 3.

An Elementary Treatise on Mineralogy and Geology; being an introduction to the study of those sciences, and designed for the use of pupils,—for persons attending lectures on these subjects,—and as a companion for travellers in the United States of America. Illustrated by six plates. By PARKER CLEAVELAND, *Professor of Mathematics and Natural Philosophy, and Lecturer on Chemistry and Mineralogy, in Bowdoin College, Member of the American Academy, and Corresponding member of the Linnæan Society of New England.*

> ....Itum est in viscera terræ:
> Quasque recondiderat, Stygiisque admoverat umbris,
> Effodiuntur opes.........................OVID.

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IT is gratifying to our national feelings to find that our men of intelligence are beginning to show themselves to the public, and to exhibit to the philosophers of Europe the state of science in the United States of America. It must be acknowledged that such efforts have been rare: that they have been too diffident of their own powers, to attempt to add much to the small stock of indigenous works, and that we have depended, perhaps too far, upon the labours of scientific men on the other side of the Atlantic. Book-making, at least when connected with physics, has not been a lucrative trade in this country; and they, who have ventured their wares in the literary market, have generally been compelled to dispose of them at low rates, and to look for their reward and the stimulus for exertion from other sources than the value of their copy-right. The Americans, it is said, are a reading people, but if the remark be true, it unfortunately happens for the interests of science, that they dip for the most part into the subjects of politics and religion, and rarely trouble themselves with the propositions of philosophy or the descriptions of natural history. This state of things however, will not long continue. The physical sciences have, within a few years, attracted much notice and been liberally encouraged; and, although time may be required to bring us up to the level of European knowledge, yet we trust that at some future period, our philosophers will be able to reciprocate the favours which they have received, and turn back the current of science to the east.

Among the branches of science which have been most generally and successfully cultivated, is that of mineralogy. The United States furnish ample materials for this study, and he who enters upon it has the satisfaction, in many instances, of being the first to discover rare, beautiful and useful minerals. There is no branch of Natural History which so rapidly rewards the labours of the cultivator. Hence a degree of zeal has been excited in the formation of public and private cabinets, and in exploring the different districts of our country, which promises not only an extension of the science itself, but, what is of more importance, a knowledge of our internal resources, which, while it increases our wealth, may also confirm our independence. Professor Cleaveland has done an acceptable service to the country by his attempt to diffuse the knowledge of this science. It was a task of no small difficulty to accommodate his work to the appetites of different readers, and it required some address to combine accurate description with popular language, and to preserve in a compendium which should be accessible to most people, a philosophic dress and an air of science. The author has very judiciously avoided an exclusive adoption of the system either of Haüy or of Werner. He was aware that "many of the writers of the two schools appear to have indulged an undue attachment to their favourite and peculiar system, and thereby to have been prevented from receiving mutual benefit, the one being unwilling to adopt what is really excellent in the other"—and the book has been rendered more valuable by the union of the descriptive method of one with the accurate and scientific arrangement of the other. This plan was followed by Brongniart, and the classification or arrangement of this mineralogist has been adopted by Mr. Cleaveland. It is founded on the results of chemical analysis, and we shall briefly state the general plan of it, in order that the reader may have an opportunity of comparing it with the more recent chemical arrangement of minerals by Professor Berzelius, which he will find exhibited in the following pages. The subjects are divided into four classes.

CLASS I.

Substances, not metallic, composed entirely, or in part, of an acid.

Order I. Acids not combined.
II. Alkaline Salts.
III. Earthy Salts.
IV. Salts with an alkaline and earthy base.

CLASS II.

Earthy Compounds, or Stones.

This class it appears is not subdivided into orders, it is composed of species which are numbered to 99.

CLASS III.

Combustibles.

This class includes 10 species.

CLASS IV.

Ores.

It is subdivided into genera, each metal constituting a genus.

It is evident, from this table, that Professor Cleaveland has taken chemical analysis as the basis of his arrangement. The propriety, however, of classifying minerals according to the nature and proportion of their elements has been questioned. Much discussion and even warm altercation have taken place respecting the best and most convenient mode of defining a species and of arranging the subjects of this science; and perhaps it may not be uninteresting to our readers, to have a concise statement of the views which have led to the adoption of the different methods, which have been followed by the great masters in mineralogy.

The greatest obstacle to the formation of a system of mineralogy has been found to arise from the difficulty of determining what constitutes a species. The mineralogists of Europe are divided in their opinions respecting the comparative merits of the French and German schools. The founders of these schools have pursued very different methods, and their arrangements are distinct and peculiar. Werner, professor of mineralogy at Freyberg, has acquired great reputation as the author of a system of classification, grounded on the natural alliances and relations of minerals. The foundation of his system is partly arbitrary, and in part laid in the results of chemical analysis. He defines a species thus, "all minerals composed of the same constituents combined in the same proportions, belong to the same species; while minerals composed of different constituents belong to different species."* Minerals were early classified into stones, salts, combustibles and ores; these are admitted by Werner, and the different classes are distinguished from each other by very obvious characters. Even the species belonging to the three last of the classes may be readily recognised. But this is not the case with the stones, and this great mineralogist, in order to simplify the whole science of mineralogy, has departed from the definition we have just quoted, and has rested the determination of the species in other grounds than the chemical composition. The constituent parts of stony bodies are so various, and they are combined in such different proportions, that a strict adherence to the composition must have introduced into the system as many species as there are substances, and even this could not have been effected without the aid of the chemist, who would find the task too Herculean to attempt it. In order, therefore, to reduce the immense variety of bodies constituting this class to some natural order, he has been guided by their *external characters*, such as *colour, form, specific gravity, hardness*, &c. When the results of chemical analysis correspond with these natural alliances of minerals, he is in some measure governed by them in determining their places in his system; when they disagree with, or contradict the conclusions derived from the external characters, they are rejected; and when neither the composition, nor the natural alliances are well defined, it is considered of little consequence in what part of the system they are arranged, before their places have been determined upon by a more rigorous investigation. It is not believed by Werner that the different species are well defined and invariably the same; on the contrary, it is supposed by an examination of the external characters, that they run into each other, and it is difficult, if not impossible to say, where one commences and the other terminates. Hence he considers it as a fact, that nature admits of a *transition* in minerals; that there are gradations, the extremes of which may present very different characters, while the passage of one into the other is almost imperceptible. This idea has been adopted and illustrated in the works of all the disciples of Werner, and may be found in the systems of Brochant and Jameson. It is singular, however, that after defining a species as Werner has done, he should immediately have departed from it, and taken as his guide a system which is almost altogether arbitrary and conventional.

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* This definition is given on the authority of Dr. Thomson of London, a staunch Wernerian.

Being governed in the determination of the species by the external characters alone, it is obvious, that he must necessarily have grouped together minerals which are essentially different in composition, and separated others which are closely connected by the nature and proportions of their constituent principles. This is the case, and this arbitrary, and, if the expression may be allowed, forced arrangement, has given a rude, unfinished and unphilosophical appearance to the system. The great object in pursuing mineralogy is, to render it useful and subservient to the wants of society, and this can in many instances be effected only by knowing the composition of minerals; and that system appears to us the best, which shall enable a man not only to recognise and name a specimen, but to state its ingredients and point out its uses.

M. Haüy, the profound crystallographer and mineralogist of Paris, has defined a species to consist of "minerals composed of the same constituents, combined in the same proportions; and having integrant moleculæ of the same form." In the first part of the definition he agrees with Werner; but in the latter clause differs essentially in making a single character the type of the species. This philosopher had successfully pursued, extended and systematised the ideas of Bergman and of De L'Isle in crystallometry, and developed a multitude of facts, which went to prove, that crystalline bodies, although they might vary in their secondary forms, yet were susceptible of division into the same integrant molecule, so long as their composition was identical. The knowledge which had thus been acquired of the structure of crystals, was applied to mineralogy, and made the basis of his system. It must be acknowledged, that this mathematical hypothesis, for such it must be considered when strictly examined, has been applied with great sagacity, and with a degree of success which could not have been anticipated. In some instances by identifying the integrant moleculæ of two minerals, he has been enabled to state their connexion and agreement in composition, before the nature of the bodies had been demonstrated by chemical analysis. Thus he inferred the identity of Chrysolite and Apatite; and the intimate connexion of the Beryl and Emerald, before the analyses of Vauquelin had proved, that the two former are composed of phosphoric acid and lime; and that the two latter contain chrome and glucina. In other cases, minerals have been correctly separated into distinct species, from the difference in form of their integrant particles, notwithstanding the most distinguished chemists could perceive no difference in their composition. Thus Arragonite and Carbonate of lime were maintained by Haüy to belong to different species, because their crystalline forms are unlike, although the analyses of Klaproth, Vauquelin, Fourcroy, Bucholz, Thenard and Biot produced nothing but carbonic acid and lime. Yet the more recent experiments of Stromeyer, and after him of Gehlen, have demonstrated, that beside carbonate of lime, this mineral contains carbonate of strontian; and it is probably to the presence of this substance that the difference in form, exhibited by arragonite may be ascribed. It is therefore probable, that some variation in the composition of ruthile and octahedrite may yet be discovered; by chemical analysis their constituent principles appear to be the same, but their integrant moleculæ are different. So far, therefore, the definition of species, as given by Haüy, seems to agree with the nature of minerals; but when we come to examine in detail, the generalization, that a species consists of a collection of bodies, the integrant molecules of which are similar, and composed of the same elements, united in the same proportions, we find it encumbered with some very weighty objections.

1. From the definition we have a right to expect, that the integrant particles should always present the same form in bodies of the same nature, and possessing the same proportion of constituents; but this is not invariably true.

2. It might be inferred, that in those minerals, which are known to be dissimilar in composition, the integrant molecules should be different; but this is so far from being correct, that there are many crystals, which are so distinct in nature, that the most unpractised eye can readily distinguish them, which yet present no difference in the forms of their particles. On the principle of Haüy, they ought to be included in the same species; but what would be thought of an arrangement, in which boracite, common salt, galena, iron, pyrites, &c. were classified together, because the integrant molecule of each is a cube? The same observations are applicable to tremolite, actynolite, and hornblende or amphibole. These substances appear to be sufficiently distinct; but because, by mechanical division, it is possible to reduce them ultimately to the same form, they have been ranked by Haüy in the same species; and consequently three bodies, one of which contains 65 parts of silex, the second 33 parts, and the third 42 parts with equally variable proportions of alumina, lime, &c. constitute one species; while others, which present analyses much less discordant, are separated from each other, merely because they do not coincide in one character. To the objection, that different minerals have the same form of the integrant particle, it has been replied by M. Haüy, that the greater number of substances which have a common molecule, may be easily distin-

guished by other characters. Are these signs to be derived from their crystallization? It is obvious that mechanical division can throw no light on their nature, since the form to which the mineralogist will arrive, must ultimately be the same in all. It is necessary then to have recourse to other means, since the crystallometrical method has failed. These means must be found in the use of the physical or external characters, and Werner must convey to us the knowledge which cannot be derived from Haüy. In fact, the remark of the latter respecting the substances above mentioned, is a tacit admission, that the form of the integrant molecule alone is not sufficient, in many instances, to guide us to a correct determination of the species; but that it is necessary to have recourse to the mechanical properties of minerals, to assign them appropriate places in a regular and consistent system.

3. But the strongest and best supported objection, which has been urged against the system of Haüy, is the necessity he has imposed upon himself, by his definition of species, of excluding all amorphous minerals, or all those substances, which are not capable of being reduced, by mechanical analysis, to some regular geometrical form. The consequence is, that he has omitted an immense number of minerals, and that his work, instead of being a system of mineralogy, is rather an illustration of the science of crystallography by models furnished by nature. "It has been said," observes Daubuisson, "that crystals are the flowers of minerals, but immense forests should go for something." With uncommon sagacity and perseverance, Haüy has sought for, and discovered these beautiful flowers; he has exhibited a rich and variegated collection, and has formed them into a splendid *bouquet* to adorn the cabinet and delight the eye of the philosopher; but Werner has ranged the forest; he has culled the wild flowers in his course, and interspersed them among the leaves and branches of a sturdier growth. The object of a system of mineralogy, is so to classify and describe the mineral productions on the surface of the globe, as that they may be easily distinguished and identified. This cannot, probably, be done by the criterion of a single character; much less, if this character be evanescent in some substances, and hypothetically supposed to be present in many others. Shall *chromate of iron* and *cerite* be denied a place in a system of mineralogy, because the forms of their integrant molecules have never been discovered? This omission would have been so palpable an absurdity, that Haüy himself was obliged to admit them, although, says Dr. Thomson, neither of the species contains a single crystallized specimen. Many other minerals are in the same predicament, and if they are to

be excluded from a system, because they are amorphous, that system must be partial and defective; it must bear the same relation to the whole science of mineralogy, that, at present, voltaic electricity does to the science of chemistry.

4. The attention which has been given to the crystalline forms of minerals, has, probably, had a bad effect on the science, as it has prevented the proper use of other characters, the knowledge of which, if not considered essential, must be conceded to facilitate the discrimination of different species. The disciples of Haüy have, in many instances, been occupied in collecting the different secondary forms of the same substance; and were this to be the case with any extensive suite of species, it is apparent, that the science must soon become overloaded with useless matter. The Count De Bournon has described forty-nine forms of arragonite, and no less than six hundred and sixteen varieties of distinct forms of carbonate of lime, belonging to fifty-nine different modifications. If it were possible to arrive at the knowledge of all the secondary forms of this mineral, the description might be tolerated, nay, perhaps be regarded as interesting, as a philosophical curiosity; but who is so presumptuous as to hope to see this done, when it has been demonstrated by Haüy himself, that if the decrements be limited to two of the simplest laws of this science, and to the abstraction of three or four ranges of particles, the number of possible forms must exceed eight millions. And what benefit, we may ask, could possibly accrue from this collection? They must all belong to the same species, and be characterized by identity of composition; the difference in external form, which, in many instances, must be perceptible only to the geometrician, arises from causes with which we are unacquainted, and which may be almost infinitely varied by nature. Haüy himself seems to have carried this description too far, and has established varieties in minerals, according to the differences perceived in their secondary forms, although, from identity of composition, they are known to belong to the same species. The consequence of this is, that he has been obliged to invent terms, and has thus embarrassed, with a new coinage, a science which was already groaning under the weight of its synonimies. Thus the *sulphate of magnesia*, or epsom salt, the composition of which is fixed and determinate, which cannot be easily mistaken, and which is usually crystallized in the form of a quadrangular prism, is divided into the bisalternate, pyramidal, triunitarian, trihexædral, equivalent, &c. &c. The carbonate of lime is subdivided into the equiaxe, the inverse, the contrasting, the mixed, the based, &c.; and the student, is overwhelmed with the weight of such terms as

enneacontraèdral, antienneacontraèdral, impair, hyperoxyde, bibisalternate, subdidistique, soustractif, soudouble, metastatique, persistant. All this is precise and excellent for the crystallometer and the geometrician, but it is of little or no use to the practical mineralogist. From making this branch of mineralogy almost an exclusive study, Haüy has been enabled to arrive at many curious facts and coincidences, and to bring the theory of crystals to a high state of refinement. In this part of his work, he is undoubtedly very superior to Werner; but, on the other hand, he has neglected the external characters of minerals, and, in this respect, has placed himself very far below the German professor. A knowledge of these characters will go much further toward the formation of a mineralogist, than the theory of lines and angles; and it has been remarked by Dr. Thomson, that "the pupils of the school of Freyberg are, beyond all comparison, more skilful in ascertaining minerals. This superiority, indeed, is generally known, and will not, I presume, be denied by Haüy himself."

The question, then, is still open for discussion, what is the surest foundation upon which the determination of a species in mineralogy may be supported? We have seen, that the use of the external characters alone must sometimes lead to error; and that the form of the integrant molecule cannot always be taken as a safe guide. Shall the species, then, in all cases, be dependent upon the true chemical composition? Let us state what is said on this subject by Professor Cleaveland.

"It must be extremely obvious, that those minerals which most resemble each other, belong to the same species. We are, then, to inquire what constitutes the most perfect resemblance between two or more minerals. Can similarity of colour, form, fracture, hardness, &c. constitute a resemblance so perfect, as that which arises from identity of composition? Or can a difference of colour, form, fracture, &c. establish so important a distinction between minerals, as that which is produced by dissimilarity of composition? Would not two minerals, both composed of phosphoric acid and oxyde of lead, in the same proportion, belong to the same species, although the colour of one should be brown, and that of the other green? Would not two minerals, composed of phosphoric acid and lime, in the same proportion, belong to the same species, although the forms of their crystals, essentially the same, should exhibit different modifications? In fine, can properties, liable to numerous variations from trivial and accidental causes, be supposed to establish the identity of two or more minerals, with that degree of evidence which is afforded by a well ascertained similarity in composition? We hesitate not to answer these questions by saying, that the *true composition* of minerals ought to be the basis of arrangement; and by this only ought the species to be determined. This only can give permanency of character to the species." p. 73. "Hence a species may be thus defined; a collection of minerals, composed of the same ingredients, combined in the same proportions." p. 74.

The author then proceeds to examine the question, whether we are sufficiently acquainted with the composition of minerals to employ it as the principal character in the determination of the species? He remarks, that the various species of alkaline and earthy salts, some species of combustibles, and almost every species among the ores of the metals, can be limited and established by their well known composition. "There remains, however, one class of minerals, composed chiefly of different *earths*, combined in various proportions, such as garnet, feldspar, &c. whose composition is not yet sufficiently understood to be employed as the basis of specific, or even generic arrangement. This extensive class of minerals is really involved in some very peculiar difficulties. Analysis can, indeed, inform us what earths are present in these minerals, and in what proportions; but it has not yet been able to discover in what manner these earths are here combined, or to distinguish between those ingredients which are essential to the composition, and those which are not, and which may, in fact, be considered as accidentally present." p. 74.

The circumstances, here mentioned, constitute, undoubtedly, the greatest objections to the adoption of the chemical system of classing minerals. In all crystallized bodies it is probable, that the constituents exist in definite proportions, and that the composition is constant and uniform for each species. There is, therefore, but little difficulty in assigning them their appropriate places in a chemical arrangement. But is this also the case with amorphous minerals? It is doubtful. In many instances, the proportions of ingredients vary in the same species of mineral which is found in different places; and it is difficult to say, how far some of them are essential, and others accidental. Some bodies appear to be formed of a number of compounds brought together, so as to constitute aggregates; and it is clear, when this happens, that the relative number and quantity of these compounds may be varied almost indefinitely; and that if the species is to be decided by the composition, the number of such species must correspond with the amount of aggregates. There would be no end to them, and all the benefits which are found to arise from a simple and comprehensive generalization, must be resigned. The art of analysis is now carried to a great extent, and a wonderful degree of precision has been introduced into this most difficult branch of practical chemistry. The application of the atomic theory, and the idea of Berzelius, that silex acts in nature the part of an acid, may, if generally adopted, do much toward the development of the true nature of aggregates, or very compound minerals, and, perhaps, bring them under some fixed laws, by which the number of species may be reduced, and a consistent arrangement be adopted.

Berzelius, of Sweden, who has done so much for chemistry, has not neglected this branch of science; he has lately presented the mineralogist with an arrangement founded upon chemical principles, and intended as a specimen of a pure chemical system. As every thing from this excellent philosopher is deserving of attention, we shall lay before our readers a concise view of his plan, premising, that the species have been omitted, from the space which they would necessarily occupy, and which, from the length of this article, we cannot spare.

CLASS I.

It consists of substances formed according to the principles of unorganic nature, that is, in which the compound bodies of the first order contain only two elements.

A. Oxygen.
B. Combustible Bodies.

Order I. Metalloids.

First Family	Sulphur
Second do.	Muriaticum
Third do.	Nitricum
Fourth do.	Boron
Fifth do.	Carbon
Sixth do.	Hydrogen.

Order II. Electro-Negative Metals.

It includes those metals, whose oxydes in combination with other bodies, perform the office rather of acids than of bases.

First Family	Arsenic
Second do.	Chromium
Third do.	Molybdenum
Fourth do.	Antimonium
Fifth do.	Titanium
Sixth do.	Silicon.

Order III. Electro-Positive Metals.

Those metals, the oxydes of which perform the office rather of bases than of acids.

Division I. *Metals whose oxydes, at a higher temperature, either alone or by the intervention of charcoal powder, are reduced and constitute the radicals of the substances formerly called metallic oxydes.*

First Family	Iridium
Second do.	Platinum
Third do.	Aurum
Fourth do.	Mercurium
Fifth do.	Palladium
Sixth do.	Argentum
Seventh Family	Bismuth
Eighth do.	Tin
Ninth do.	Lead
Tenth do.	Copper
Eleventh do.	Nickel
Twelfth do.	Cobalt
Thirteenth Family	Uranium
Fourteenth do.	Zinc
Fifteenth do.	Iron
Sixteenth do.	Manganese
Seventeenth do.	Cerium.

Division II. *Metals which cannot be reduced by charcoal powder, and whose oxydes form the earths and alkalies.*

First Family	Zirconium
Second do.	Aluminium
Third do.	Yttrium
Fourth do.	Glucinum

Fifth Family	Magnesium
Sixth do.	Calcium
Seventh do.	Strontium
Eighth do.	Barytium
Ninth do.	Sodium
Tenth do.	Potassium.

CLASS II.

Contains bodies formed according to the principles of organized nature, that is, in which the compounds of the first order contain more than two elements.

Order I. Evidently putrefied organic bodies.
Humus
Turf
Brown Coal.

Order II. Resinous Bodies.
Amber
Retinasphalt
Mineral Caoutchouc.

Order III. Liquids.
Naphtha
Petroleum.

Order IV. Pitchy Bodies.
Maltha
Asphalt.

Order V. Coals.
Branderz
Stone Coal.

Order VI. Salts.
Sulphate of Ammonia
Sal Ammoniac
Mellite.

The great peculiarity in this system is the classing of silicon, or the basis of silex, among the acidifiable metals. The general opinion now entertained among chemists, is, that this basis is not metallic; and Sir Humphrey Davy, who, in his Elements, has ranked it in that class, now thinks it more analogous to boron, and that it should, of course, be placed among the acidifiable unmetallic bases. This arrangement, in fact, would strengthen the notion of Berzelius, of its performing the part of an acid. He has formed genera of minerals, which he calls *siliciates;* and they include numerous species, particularly in the earthy families; for example, in that of *aluminium*, sapphire, ruby, corundum, emery, pitch stone, staurolite, &c. are regarded as siliciates; and, under the head of calcium, we find the same name given to lomonite, mealy zeolite, prehnite, chrysoberyl, asbestos, actynolite, coccolite, black garnet, &c. &c.

It appears, then, that three different methods have been employed in determining the species, and in forming a system of classification for the subjects of mineralogy; the external characters of Werner; the integrant particle and composition of Haüy, and the chemical constitution of the chemists. With regard to the permanency of any arrangement, the advantage is undoubtedly on the side of the chemists; for the elements, and the proportions in which they enter into the composition of a mineral, being once known, it is easy to assign it an appropriate place in the system. But the question may still be asked, what is the best method to recognise a mineral? Or, in other words, how is a person, who has just commenced the pursuit of mineralogy, to identify a specimen? He will be decided, without doubt, principally by the external characters, by comparing it with the descriptions which he reads in books, and with other specimens to which it bears a resemblance; and he will, afterwards make the simple experiments with acids, the blow-pipe, the magnet, and the electrometer, which are pointed out to him by the authors. In judging of minerals, a person decides more by the external characters, than, perhaps, he is aware; and hence the chief reason, why a beginner is particularly anxious to accumulate specimens, that he may acquire a practised eye. The pure, scientific arrangement of his minerals, is a different thing, and requires an intimate knowledge of practical and analytical chemistry.

We have occupied so much space with the preceding observations, that we have, unfortunately, but little room to spare on the subject of geology. Under this head, Professor Cleaveland has given concise, but very interesting views of the structure of the earth, of the theories of Hutton, and of Werner, and of the constitution and formations of the different rocks. There has been much science, ingenuity, and fancy displayed by authors in the discussion of the question, whether minerals and rocks have been deposited from aqueous solutions, or have been formed by igneous fusion from disintegrated rocks and mountains. Werner lives in Germany, and circumstances induced him to believe, that, at some former period, the globe was covered with water; that this water held in solution various matters, which were deposited in succession, and gave rise to the different *formations*; that some were separated before animals and vegetables existed, of couse they contained none of their remains, and were called primitive; that the next deposition took place just after organized beings were called into existence, and, therefore, exhibit some traces of them, whence this formation is called transition; and, finally, at the third deposit, nature then abounding in animals and vegetables, many of them were enveloped in the descending earth, and gradually petrified; hence the frequency of organic remains in rocks of this kind, which were named secondary. Dr. Hutton resided in Scotland, where there is abundance of whin stone, trap and basalt; and he contended for the igneous origin of the globe. The rocks and mountains are gradually worn down, or disintegrated by the action of the common elements, and carried to the bottom of the sea; there, by the operation of subterranean fire, the mass is melted, or softened, and agglutinated, and is to constitute the materials of new mountains and continents. Such are the noble speculations of philosophers. The disciples of these schools, in order to find arguments to support their favorite hypotheses, have been led to examine minutely into the structure of the globe, and many curious and interesting facts have been brought to light. Independently of the dogmas of the Wernerian and Huttonian theories, these furnish a very fine groundwork for future research; and, even at present, the American geologist must contemplate, with much pleasure, the map of the United States which enriches the volume of Professor Cleaveland, and which exhibits, at one view, from the labours of the celebrated Mr. M'Clure, the position of the different formations in our extensive country.

It would be idle for us to attempt a regular analysis of this work. We shall, therefore, leave it for the examination of the reader. He will find it to be the production of a man of science, who is capable of writing clearly, and of describing accurately; of one who has brought down the science to the time at which his book was printed; who has not done himself justice in stating the number of works which he has consulted, and whose work is recommended to all those who are engaged in the pursuit of mineralogy, as containing, in a moderate compass, the essence of Werner, Haüy, Jameson, Brochant, and Brongniart.

INTELLIGENCE.

Domestic.

Massachusetts Medical Society.

THE annual meeting of the Society was held on the first Wednesday in June, and honoured by the attendance of his Excellency Governor *Brooks*, and a great concourse of respectable physicians. The discourse was delivered by Dr. Orr, of Bridgewater. He took, for his subject, a comparative consideration of the functions of animals and vegetables, and exhibited great ingenuity and learning, in the attempt to prove that all the functions of animated beings are to be found existing in the vegetable world.

The Counsellors elected officers for the next year, the following gentlemen, viz.:

For the *District of Suffolk.* Doctors Lemuel Hayward, David Townsend, Thomas Welsh, Aaron Dexter, William Spooner, Isaac Rand, Jr. Asa Bullard, John G. Coffin, John Dixwell, James Jackson, Benjamin Shurtleff, John C. Warren, John Gorham, William Gamage, Jr.

Essex. Doctors Edward A. Holyoke, Joshua Fisher, Thomas Kitteridge, Benjamin L. Oliver, John D. Treadwell, Oliver Prescott, James Gardner, Nathaniel Bradstreet.

Middlesex. Doctors JOHN BROOKS, Josiah Bartlett, Isaac Hurd, Joseph Fiske, Amos Bancroft, Calvin Thomas, Abiel Heywood, Rufus Wyman, James P. Chaplin.

Worcester. Doctors Oliver Fiske, Abraham Haskell, Austin Flint, Jonathan Osgood, Israel Whiton, Stephen Batcheller, Jr.

Hampshire. Doctors Peter Bryant, Estes Howe, John Stone, Elihu Dwight, William Sheldon, Enos Smith.

Berkshire. Doctors Timothy Childs, Hugo Burghardt, Asa Burbank.

Norfolk. Doctors Amos Holbrook, James Mann, John Bartlett, Abijah Richardson, Nathaniel Miller.

Plymouth. Doctors James Thacher, Daniel Shute, Hector Orr, Nathan Hayward.

Bristol. Doctors Samuel Perry, Benjamin Billings.

York. Doctors Joseph Gilman, Richard C. Shannon.

Cumberland. Doctors Ammi R. Mitchell, Gad Hitchcock, John Merrill.

Kennebeck and Somerset. Doctors Daniel Coney, James Parker, Benjamin Page, Ariel Mann.

Lincoln, Hancock, and Washington. Doctors Samuel Adams, Benjamin Brown, John Stockbridge.

At a stated meeting of the Counsellors held on the fifth day of June, A. D. 1817, the following officers were elected for the year ensuing, viz.:—

Joshua Fisher, M. D.	*President*
Thomas Welsh, M. D.	*Vice President*
John C. Warren, M. D.	*Corresponding Secretary*
John Dixwell, M. D.	*Recording Secretary*
John G. Coffin, M. D.	*Treasurer*
William Gamage, Jr. M. D.	*Librarian*
David Townsend, M. D.	*Censors*
Aaron Dexter, M. D.	
Josiah Bartlett, M. D.	
William Spooner, M. D.	
James Jackson, M. D.	
Oliver Fiske	*Censors for Worcester District Society*
Abraham Haskell, M. D.	
Jonathan Osgood	
Austin Flint	
John Green	

Copy from Records—By order,
JOHN DIXWELL, *Recording Secretary.*

American Medical Botany.

Dr. Bigelow, Professor in Harvard University, on the foundation of Count Rumford, has just issued proposals for the publication of the American Medical Botany, announced in the New England Journal of October, 1816. A work of this kind, if well executed, will be extremely useful to the physicians of this country, and interesting to naturalists here and in Europe. Dr. Bigelow is sufficiently known to the public, as the author of the "Florula Bostoniensis," a production, which does honour to the science of this country; and also as the editor of an American edition of Dr. Smith's Botany, to give them an opportunity of judging of the probable execution of the work proposed. The writer of this notice has accidentally seen some of the coloured engravings, which are beautiful, and appear to be correctly done. As publications of this kind are expensive, it is to be hoped that the patronage of men of science will be liberally given, to encourage the author to the completion of his plan. The following is the prospectus:—

Cummings & Hilliard, Boston, have in the press and will speedily publish, American Medical Botany, being a collection of the Native Medicinal Plants of the United States, with coloured engravings. By Jacob Bigelow, M. D. Rumford Professor, and Lecturer on Materia Medica and Botany in Harvard University.

This work has been for some time in preparation, and a part of the engravings were completed more than a year since. Its intended publication was announced in the New England Medical Journal for October, 1816. It is undertaken with a desire to promote the knowledge of those indigenous plants, which possess medicinal properties, and which are, or may be applied to useful purposes. The subject opens a field of research, which, if carefully pursued, must afford results important to the medical profession, and useful to the country. The present work is offered rather as the commencement of such an investigation, than as the exposition of a complete state of knowledge on the subject. Its principal object will be to distinguish what is known or satisfactorily established on the subjects it comprises, from what is uncertain and dependent on vague report or insufficient evidence.—To the cultivators of botany, this work is presented as a serviceable auxiliary, and as a specimen of the arts, it will probably be found superior to any thing of the kind, which has been executed in the country.

The work will be published in half volumes, on fine paper of the royal octavo size.

Each half volume will contain ten coloured engravings from original drawings, copied from nature, and executed in a manner not inferior to those in Woodville's Medical Botany, or Curtis's Botanical Magazine; together with dissections for the use of botanical students.

Each half volume will comprise about one hundred pages of letter press, containing the botanical character and history of each plant, its places of growth and period of flowering, its *preparations* and uses in medicine, diet, and the arts, as far as they are known to the author from his own experience, or the testimony of those on whom he can depend.

The price to subscribers of each half volume in boards, will be two dollars and fifty cents. The work will probably consist of three volumes, which number only, subscribers will be obligated to receive. They will be published in as quick succession, as may be found consistent with their faithful execution.

Boston, June, 1817.

Mr. Hare's Blow Pipe.

As a friend to the reputation of our country, in science, as well as to its other important interests, I presume you will be willing to publish the following statement of facts.

Various notices, more or less complete, chiefly copied from English newspapers, are now going the round of the public prints, in this country, stating, that "*a new kind of fire*" has been discovered, in England, or at least, new, and heretofore, unparalleled means of exciting heat, by which gems and all the most refractory substances in nature are immediately melted, and even in various instances, dissipated in vapour, or decomposed into their elements. The first glance at these statements, (which, as regards the effects, I have no doubt are substantially true,) was sufficient to satisfy me, that the basis of these achievements was laid by an American discovery, made by Mr. Robert Hare of Philadelphia in 1801. In December of that year Mr. Hare communicated to the Chemical Society of Philadelphia, his discovery of a method of burning oxygen and hydrogen gases in a united stream, so as to produce a very intense heat. In 1802, he published a detailed memoir on the subject, with an engraving of his apparatus, and he recited the effects of his instrument, some of which, in the degree of heat produced, surpassed any thing before known. In 1802 and 1803, I was occupied with him in Philadelphia, in prosecuting similar experiments, on a more extended scale, and a communication on the subject was made to the Philosophical Society of Philadelphia; the memoir is printed in their Transactions; and Mr. Hare's original memoir was reprinted in the Annals of Chemistry, in Paris, and in the Philosophical Magazine in London. Mr. Murray, in his system of Chemistry, has mentioned Mr. Hare's results in the fusion of several of the earths, &c. and has given him credit for his discovery.

In one instance while in Europe, in 1806, at a public lecture, I saw some of them exhibited by a celebrated professor, who mentioned Mr. Hare as the reputed author of the invention.

In December, 1811, I instituted an extended course of experiments with Mr. Hare's blow pipe, in which I melted lime and magnesia, and a long list of the most refractory minerals, gems, and others, the greater part of which had never been melted before, and I supposed that I had decomposed lime, barrytes, strontites, and magnesia, evolving their metallic bases, which burnt in the air, as fast as produced. I communicated a detailed account of my experiments to the Connecticut Academy of Arts and Sciences, who published it in their Transactions for 1812; with their leave it was communicated to Dr. Bruce's Mineralogical Journal, and, it was printed in the 4th Number of that work. Hundreds of my pupils can testify, that Mr. Hare's splendid experiments, and many others performed with his blow pipe fed by oxygen and hydrogen gases, have been for years past annually exhibited, in my public courses of chemistry, in Yale College, and that the fusion and volatilization of platina, and the combustion of that metal, and of gold and silver, and of many other metals; and the fusion of the earths, of rock crystal, of gun flint, of the corundum gems, and many other very refractory substances, and the production of light beyond the brightness of the sun, have been familiar experiments in my laboratory. I have, uniformly, given Mr. Hare the full credit of the invention, although my researches with his instrument had been pushed farther than his own, and a good many new results added.

It is therefore with no small surprise that in the Annales de Chimie et de Physique for September, 1816, (a French Journal) I found a translation of a very elaborate memoir, from a scientific Journal, published at the Royal Institution, in London, in which a full account is given of a very interesting series of experiments performed by means of Mr. Hare's instrument, or one on the same principle, but without any notice being taken of Mr. Hare's invention or experiments or of mine, and the whole is exhibited as original. On a comparison of the memoir in question, with Mr. Hare's and with my own, I find that very many of the results are identical, and all the new ones are derived directly from Mr. Hare's instrument with the following differences. In Mr. Hare's, the two gases were in distinct reservoirs to prevent explosion; they were propelled by the pressure of a column of water, and were made to mingle, just before their exit, at a common orifice. In the English apparatus, gases are both in one reservoir, and they are propelled by their own elasticity, after condensation by a syringe.

Professor Clark of Cambridge University, the celebrated traveller, is the author of the memoir in question, and we must presume that he was ignorant of what had been done by Mr. Hare and myself, or he would candidly have adverted to the facts.

Measures have been taken to set this matter right in Europe, but, in the mean time, whatever treatment the subject may receive there, it is proper that the American public should know, that Mr. Hare was the inventor of the instrument, with which in Europe they are now performing the most brilliant and beautiful experiments, and that there are very few of these results hitherto obtained there, by the use of it, (and the publication of which has there excited great interest,) which were not, several years ago, anticipated here, either by Mr. Hare or by myself.

As I have cited only printed documents, or the testimony of living witnesses, I trust my countrymen will not consider this communication as indelicate, or arrogant, but simply a matter of justice to the interests of American Science, and especially to Mr. Hare.

BENJAMIN SILLIMAN.

Foreign.

New Method of Curing Cancerous Diseases.

In the different interviews with M. Heschel, Surgeon of the Austrian Military Hospital, established at Colmar, he informed me of a successful method which he employs in cancerous ulcers of the face. He assured me, that before his departure from Vienna, he had completely cured by it a man of rank, aged seventy, suffering from cancer of the malar region, which was spreading rapidly towards the nose, and had baffled the skill of the ablest surgeons of Vienna. The following is his process:—He touches the scirrhous inverted margins of the ulcer with nitric acid (*acide nitrique fumant.*) For this purpose, he employs a golden probe, as all other metals are oxydized by the acid. He passes the probe, moistened with the acid, slightly over the borders of the cancer, and repeats the process two or three days successively. It is discontinued, when the inflammation rises high; and then reapplied till the borders are destroyed by sloughing, and the ulcer is reduced to the state of a simple sore. This invariably happens where the disease is merely local. The dressing consists only of lint, moistened in a mixture of water, gum arabic, and brandy.

A short time since, I recommended this process to a man, aged sixty-eight, of mucoso-nervous constitution, who had a cancerous ulcer on the lower part of the right leg. Its borders were callous and inverted; the sanies, discharged from it, excessively fœtid, and the attendant pains unusually severe. The ulcer spread in all directions, notwithstanding the efforts of an experienced surgeon. The nitric acid, employed on the plan of M. Heschel, caused the scirrhous borders to slough. The wound, rendered simple, was merely dressed with adhesive straps according to the English method, of late, and most deservedly eulogized in France: and the cancerous ulcer is, at this moment, completely healed. The plan of M. Heschel is certainly entitled to the attention of professional men.

Neuralgia of the Spermatic Cord, cured by Moxa.

A man, aged thirty-five, of a nervous lymphatic temperament, had been subject, from the age of twenty, to a slight eruption, consequent on itch, and frequently recurring in winter. At the age of twenty-five, a violent pain of the right temple, returning every morning from ten o'clock till noon, was removed by the application of a blister to the nucha. During his twenty-ninth and thirtieth years, he felt, at times, tearing pains in the left epididymis and spermatic cord. They attracted little attention. He lived very regularly. At the age of thirty, the pain became more violent, and considerable testicular inflammation ensued; and was subdued, after three weeks, by leeches, poultices, and diluents. Thenceforth, the pain was uninterrupted, but variable in severity. In the worst paroxysms, commonly brought on by erections and involuntary nocturnal emissions, the pain radiated to the breach, to the left thigh and leg, in the course of the vas deferens, to the basis of the bladder and urethra. It induced frequent desire to void urine, with a sense of scalding. Sometimes it became so violent, that the patient lost his appetite, was low spirited, wished for the extirpation of the testis. The organ was more or less swollen, according to the severity of the pain. A caustic on the thigh was of no service. Leeches, poultices, anodynes in every form: plasters of opium and cicuta, and other topical applications of that class aggravated the disease. Mercurial frictions, employed under erroneous suspicions of a venereal taint, and the anti-psoric treatment, had a similar effect. Ice applied to the spermatic cord, produced great, but imperfect relief. The pain was diminished by a blister, so long as the cuticular inflammation lasted. A seton was introduced with the same results. Vinous infusions of valerian, bitters, and antispasmodics rendered the pain more severe.

The patient then renounced all topical means, except frictions with sulphuric ether, from which he derived momentary relief, and only kept open an issue, and wore a suspensory bandage. The testicle and epididymis retained the natural volume, and, by dint of strict regimen and attention, the pains became tolerable; but they immediately re-existed by the slightest deviation in regimen. Under these circumstances, moxa was applied on the painful part. The pain, at first, ceased; but recurred slightly on separation of the eschar. A second, applied fifteen days afterwards, again removed the pain; and was again felt when the slough was detached: yet only by interervals, and withal, so slightly, that the patient paid no attention to it.

Bath Literary and Philosophical Society.

A paper by Dr. Wilkinson, on the Rise of Fluids in Capillary Tubes was afterwards read. Its object was to show, that the experiments on this subject do not accord with the theoretic calculations of Professor Atwood of Cambridge, and other philosophers;—that the results do not correspond with any fixed rule, but are entirely dependent on certain conditions of the tube; and that no dependence can be placed on any deductions from mathematical investigations as to what has been termed *capillary attraction.*—On this subject, we have been for some time making experiments, the result of which is, that the supposed filtration by capillary attraction will extend no further than the syphon; that is, that the filaments on the outside of the vessel must be brought lower than the fluid within.

Anatomy of the Brain.

Dr. Spurzheim's Second Demonstration of the recent Brain, as we remarked in our last, took place at the Anatomical

Theatre of St. Bartholomew's. The lecturer's principal object was to demonstrate those facts which had been disputed or contradicted by his adversaries. He showed, for instance, that there is brown matter in the interior of the cerebellum, in the pons varolii; and that there is no part of the brain in which the same may not be discovered. The decussation of the anterior pyramids was carefully pointed out, and the structure of the cerebellum, more particularly the variety of appearances which the corpus dentatum exhibits, according to the manner in which it is cut or examined. It should be remarked, that Dr. Spurzheim begins his examination at the base of the brain, and demonstrates the different parts in their connexion without slicing and destroying them. The pons varolii, and most of those parts which have acquired a number of whimsical names, he distinguishes as origins and commissures of the various parts of the brain. This very much assists the memory, by a perpetual appeal to the judgment, instead of requiring the constant recollection of an unsatisfactory nomenclature. The transverse fibres of the pons are considered as the means of uniting the two hemispheres of the cerebellum, while the longitudinal bundles of the pons bring the parts of the brain proper, into communication with the medulla oblongata. In the corpus callosum he demonstrates the fibres running sideways in a most beautiful and satisfactory manner, by gently scraping with the scalpel: showing, at the same time, that a longitudinal scrape exhibits only a smooth appearance. The corpus callosum proves the commissure or uniting medium of the two hemispheres and their convolutions. Some of these were unfolded with the greatest delicacy, to show the manner in which that organ is disposed so as to occupy the least possible space with the largest surface. So delicate is the structure of the whole, that it would have been impossible not to suspect some slight of hand in the division of the white substance into two layers, had not the lecturer had recourse to the blow pipe, the force of the air from which was sufficient to exhibit the natural division in the most satisfactory manner. This led to the explanation of those causes by which an hydrocephalic subject retains his senses, his powers of reflexion, his affections, and faculties. By the slow manner in which the water increases, the layers are divided; and, if the water still increase, the fibres gradually are directed in an horizontal instead of a vertical direction. Thus the brain retains its full bulk, as has been proved by weight, where it has been expanded so thinly round the enlarged cranium as to have been suspected to be altogether, or nearly all, absorbed. With much candor Dr. Spurzheim informed his hearers that the existence of the brain in hydrocephalic heads had been noticed by the accurate Morgagni, who blames some of his predecessors and contemporaries for having overlooked the brain in its expanded state. Dr. Spurzheim assures us, that, however thin the brain may appear, or even though it may be thought to have disappeared altogether, yet, that in one such case he had weighed it against the brain of subjects of the same age, and found no difference.

The following account of the examination of the cranial contents, in two subjects approaching to idiotcy, are worth recording; and we hope will prove the means of accumulating other facts in illustration of the properties of this important organ.—These are from the "Repository."

A man, aged 60, approaching the condition of an idiot, tall, and very thin, had been affected for three years with giddiness and pain in the head, which had from time to time been relieved by local bleeding, blisters, and setons to the neck; but most by a caustic applied at the point where the coronal and sagittal sutures meet. He always had, combined with this affection of his head, more or less of griping with slimy stools; which latter symptoms, for the last twelve months, had been particularly severe, producing an almost constant diarrhœa and tenesmus.

* * * * *

"*Sectio cadaveris*, eighteen hours after death.—*Head.* Nothing remarkable was observed in the scalp or dura mater. On removing the latter, the tunica arachnoidea was found thickened and opake, and, on puncturing it, a serous fluid, to the amount of about three ounces, escaped from between it and the pia mater. Some of the vessels of the pia mater were nearly empty, and appeared to be larger than common. There was about an ounce of serous fluid in the lateral ventricles. Both the cineritious and medullary substance of the brain, when cut into, had a more vascular appearance than in the natural state of that organ. The object the most curious was an anomaly which prevailed in the structure of the brain: the posterior cornua of the ventricles were absolutely wanting, so that there was no trace of the hippocampus minor. This deviation did not appear to be the effect of disease; for the structure of the brain did not differ in any other respect from that disposition of parts which is common in other brains."

"The singularity in the construction of the brain in the above case, with its attendant weakness of intellect, struck me so forcibly, that I determined to embrace every opportunity that offered of minutely examining the cranium of every patient where there was an approach to idiotism.

"It was not long before a man, aged twenty-five, and who was little superior in understanding, died from erysipelatous inflammation attacking the head. Upon examining the structure of the lateral ventricles, the anterior and interior cornua appeared fully developed; but here, as in the first case, the *posterior* cornua were wholly deficient."

These histories may make part of future records. Though the surgeon discovered no other difference in the two brains than the want of *posterior cornua*, yet when we consider the very imperfect manner in which the demonstration of the brain has been hitherto conducted in our best anatomical schools, we may, without breach of candour, presume that a Gallite would have perceived other peculiarities. Dr. Spurzheim assures us, that though, for the most part, every brain consists of the same exterior general form, yet, in marking minutely the outlines, there is found as great a variety as in the various features of the human face. He finds no two brains exactly alike, any more than two faces.

All Professor Davy's recent experiments with damaged flour seem to mark carbonate of magnesia as the proper remedy. The quantity used is from 20 to 30 grains to ℔j. of flour.

Tests of Arsenic.

One of those trials has lately taken place for murder which so often astonish us, by proving how little attention is paid to the most important phenomena of the stomach, in cases which are decided concerning the life of a suspected person.

The prisoner, Mr. Robert Saul Donnall, a surgeon, practising at Falmouth, was indicted for no less a crime than that of destroying his mother, by giving her arsenic with her food, at his own house. Dr. Edwards was consulted during her short illness, and gave the following deposition.

"Dr. Richard Edwards gave evidence, that he had been sixteen years in the profession. When called in between four and five o'clock on the morning of the 4th of November, he asked the prisoner some questions respecting Mrs. Downing: the prisoner said that she had an attack of the cholera morbus; and that she had been similarly affected about a fortnight before. The witness went into her room, and found that she required a little rousing before she could answer any questions: she complained of great heat, and her pulse was frequent and fluttering: she had no sickness at that time, but was in very great danger. He understood, from the prisoner, that he had given her an opening medicine, an emetic, a saline draught, and some opium. The witness wrote a prescription, with a view to remove something offensive which appeared to be in her stomach or bowels. He had never met with a case of cholera morbus that had produced death in a shorter time than three or four days; and he was of opinion that that disorder was not the cause of the death of Mrs. Downing. On Thursday the 7th of November, the witness was requested to go and examine the body, by opening it, having previously heard of the suspicion of poison. He saw the prisoner at the house, and a surgeon of the name of John Street. After taking the body out of the shell, the witness perceived that the prisoner was tucking up his sleeves and preparing to open the body; but the witness told him that he must have nothing to do with the operation. On opening the stomach, the contents were poured into a basin, and, after the lapse of a few minutes, they were examined, but no deposit of any thing heavy or solid could be discovered. The stomach was in a state of stellated inflammation, and both the coats of the stomach were softened, as if by the action of some corrosive substance; one of them could be scraped off by the finger nail. The blood vessels of the stomach were more than ordinarily turgid, but the liver and lungs were sound: the heart the witness did not examine. The basin with the contents of the stomach had been carefully placed on a chair; witness observed at the time, aloud, that they must be preserved, and the prisoner must have heard it. They then proceeded to inspect the intestines, which were also inflamed; and the witness felt certain that it could not have been produced otherwise than by the operation of an active poison.—After this investigation, the witness turned round to the basin, and was surprised to find it empty: he asked the prisoner what he had done with the stomach, and he replied, that he had thrown them into the chamber-vase, where was a quantity of water, with which the intestines had been washed. The witness said, that it would give him a great deal more trouble in making experiments to ascertain if any poison were in the stomach, as he must evaporate about two quarts of water. Mr. Street took charge of the chamber-vase and its contents; it was pushed under the bed. The witness afterwards applied to it various chemical tests, and they indicated the presence of arsenic in a fluid, but he could not detect it in a solid state. He had not the slightest doubt that arsenic was the cause of the death of Mrs. Downing, and he should have been of that opinion had he not analyzed the contents of the stomach."

"Dr. Edwards, on being recalled by Mr. Justice Abbot, said, that 80 parts of cold water would dissolve one part of arsenic; that warm water would dissolve a larger proportion, and that then two or three tea-spoonsful, or a table-spoonful, would probably produce death."

Is it possible not to feel surprised at the boldness of the physician who should have no doubts on such slender evidence? The stellated inflammation, as it is called, was probably no more than blood-vessels denuded by the action of the gastric juice on the surrounding parts; and the surface of the stomach, softened so as easily to be scraped off, was the regular process of digestion, as described by Mr. Hunter, when it takes place after death. Why did not Dr. Edwards put into a dead stomach some of the solution of arsenic, of which, he says, a table-spoonful would be enough to produce death? or why did he not mix some such solution with the contents of Mrs. Downing's stomach, and see how far a *corrosion* would have taken place. Corrosion is a chemical process, and should follow the application of a corrosive substance to dead as well as living animal matter. Inflammation may be excited by a variety of causes; and, in all sudden deaths with food in the stomach, we should be prepared to expect a partial digestion of the stomach. Of this we shall say more when our experiments on oxalic acid are completed.

A caution.—The following accident happened at Munich on the 12th of February:—An apothecary's shopman being engaged in beating up, in a mortar of serpentine stone, a mixture of oxymuriate of potash, sulphur, sugar, and cinnabar, for the purpose of making chemical matches, a terrible explosion took place, which killed the person who was making the mixture, wounded the apothecary, who at that instant entered, blew the mortar to pieces, and damaged the stove and furniture of the room.

At a recent sitting of the Society of Medicine of Paris, M. l'Espagnal communicated a very singular fact:—A workman of a village at a considerable distance from the capital, in barking the branches of trees, totally cutoff the last phalanx of the index finger of the left hand, and went home; his mother had the idea of replacing the part cut off, and went and found it amongst the chips where he had left it, and eighteen minutes after the accident she applied the parts together, and they united perfectly, but remained in a state of atrophy for some time, but by slow degrees it recovered its sensibility.

Numerous cases have recently been quoted in the various Medical Journals of Europe, of examples of uniting by a suture, and in some cases by the first intention; but in, we lieve, all these cases the part was not entirely cut off, but hung by a particle of the skin. This case, well authenticated, proves that this is not absolutely necessary to insure success.

Dr. Husson, who had so greatly contributed to the propagation of vaccination in France, found, recently, on opening the body of a woman at the Hotel Dieu, a *polypus* in the stomach; it measured eight inches in length, and six-tenths of an inch in diameter. This body, which is of a greater size than any hitherto discovered, adhered to the membrane of the stomach, but it did not appear to have had any action in the vital functions, nor had it any influence in the cause of her death. Dr. Brechet has made a drawing and preparation, with a description, of this polypus.

A suture of the membrane of the stomach has been the subject of a memoir read at the Faculty of Medicine, by the Baron Percy. The success with which this operation has been performed in more than one case, has removed all apprehensions of these wounds being invariably mortal, as they had previously been considered.

On the Venom of the Viper. Extracted from the Discorso del Sig. Prof. Mangili, *intorno al Veleno della Vipera, letto al R. I. Instituto.*

The ancients were of opinion that the poison of vipers, introduced direct into the alimentary canal, was not dangerous. They founded their argument on the fact, that one might with impunity suck the wound made by a viper, carefully spitting it out; and which was one of their remedies. Redi has adopted this opinion.

Since that period, Fontana advanced that, if a small dose of poison might be taken by man without danger, on account of his bulk compared with that of the viper, a larger dose would be dangerous, and even fatal. He cut off the heads of eight vipers, and expressed all the venom into a tea-spoon, and introduced it into the stomach of a pigeon, which had had no food for eight hours. In less than a minute the animal appeared weak, in two minutes more it began to stagger, fell on its side, and died in six minutes in strong convulsions.

This experiment is the reverse of that of Redi, who, having diluted the poison of four vipers in a glass of water, and having given part to a goat and part to a duck, perceived no effects resulting from it.

At length, Jacob Strozzi drank with impunity the poison of a viper diluted in half a glass of wine: at another time he drank the poison of three vipers, diluted in a similar manner.

Wishing to clear up the point, M. Mangili took four small blackbirds. The first drank the fluid venom of three vipers, the second that of four, the third that of five, and the fourth that of six, of these animals. They appeared at first plunged into a state of stupidity and inaction, but in less than an hour they were as lively as before, and ate with appetite.

He then gave the venom of upwards of twenty vipers to a little puppy, in which it produced no effect.

These experiments were so conclusive, that a person present swallowed the venom of four large vipers without being affected by it.

The following year the experiment was repeated on a crow, which had fasted twelve hours: it drank with impunity the venom of sixteen vipers.

In October, 1814, says the author, I forced seven large vipers to shed all their venom into a cup. I steeped in it crumbs of bread, and made a pigeon swallow them. At first it appeared ill, but soon recovered itself. Some days afterwards I introduced into its food, as well as that of another pigeon, a small quantity of venom, very dry, which had been kept fourteen months in a glass hermetically closed. They both manifested signs of being poisoned, and died in about two hours.

Another pigeon swallowed the poison of ten large vipers without it at all affecting it.

Fontana asserted, that dry poison lost its virtues in nine months. The above experiment shews the contrary. In introducing poison into a wound, I cover the wound with court plaster, in order that the poison may not be rejected by the flowing of the blood.

I have made similar experiments on pigeons with poison kept eighteen, twenty-two, and even twenty-six months. The effect was always the same; and therefore I have no hesitation in contradicting the assertion of Fontana, and to affirm, that poison, carefully kept, will retain its noxious qualities for years.

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The New-England Journal

OF

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Vol. VI. OCTOBER, 1817. No. IV.

Observations on Opthalmia. By FRANCIS MOORE, M. D.

[Communicated for the New England Journal of Medicine, &c.]

ACUTE opthalmia of a purely phlegmonous character, when treated on general principles at an early period, is easily cured. I speak of that species, which is produced by a blow on the eye, exposure to intense light, the application of cold, that which is induced by long exercise of the eye, &c. This kind readily yields to the application of leeches, cold lotions, &c. &c.

But every medical man, of much practice and observation, must have met with cases which resist all the ordinary means for reducing inflammation.

Having met with some cases of severe acute opthalmia, in which I felt deeply interested, I was led to look into the practice of some of my medical brethren, and found them equally refractory under their treatment. In the management of the cases alluded to, I was induced to suppose, that the local inflammation was aggravated by an inflammatory diathesis of the system, and had recourse to saline purgatives; but without effect.

From the ill success of these means to subdue local inflammation, I think we may correctly infer, that in these cases, it was complicated with some morbid irritation.

As a great number of cuticular eruptions are supposed to arise from biliary derangement, I was disposed to refer this to the same origin.

1st. From the extreme delicacy of that portion of the skin, which is reflected over the eye, giving it a predisposition to be thus affected.

2d. From the saffron hue of the conjunctiva, being one of the first symptoms in icterus.

After the adoption of these sentiments, I superadded to the other means, the frequent use of calomel cathartics, (directing their exhibition every second or third day) and found them fully adequate to the removal of all the symptoms.

I should deem it a matter of some consequence to be able to point out some pathognomic symptoms of this species of opthalmia, by which it might be readily discriminated. The only appearances, which I have found to be peculiar to this affection, are a very troublesome degree of itching, and an extremely irritating discharge from the eye.

I think I have met with this affection more frequently in persons of debauched habits, in whom the functions of the liver are disordered by the free use of ardent spirit, and, in some cases, of females who were subject to habitual costiveness.

There is one other variety of opthalmy, which, in a great majority of cases, resists the ordinary method of treatment. This is commonly denominated chronic, depending on a weakness of the vessels of the conjunctiva.

That the principal indication in the treatment of this affection is to restore the tone of the enfeebled vessels, is readily admitted; and, for this purpose, the whole catalogue of astringents have been recommended and employed.

However plausible the operation of astringents may appear in theory, they frequently disappoint the expectation of the practitioner. In reflecting on this subject, I think more has been expected of them, than is in their power to perform. To produce any important effect, astringents must be long applied to the part, but who does not know, that any extraneous matter, introduced into the eye, is immediately washed out by an increased flow of tears. Doctor Barton, in his lectures, speaking of chronic opthalmy, observed;—"I know of no remedy, on which much reliance can be placed."

From the failure of astringents, I was led to try the means used for curing varicose veins in other situations, and, for this purpose, had recourse to the expedient of tying a folded silk handkerchief, moderately tight, across the eyes at bedtime, to be worn during the night. By this means all light is excluded, the eye is gently fixed in the orbit, and the slight degree of compression gives the vessels an opportunity of recovering their tone. The experiment has fully answered my expectation, and from the number of cases in which I have used it successfully, I can very confidently recommend its adoption.

The importance of this remedy will be better understood, when we consider that, in this enlarged state of the vessels of the conjunctiva, the eyelids are imperfectly closed; so that sufficient light is admitted to be a cause of irritation. In sleep (which is the repose of the senses) the eye, in a diseased state, cannot fully participate; it is exposed to the stimulus of light ab extra, and is obedient to fancy in dreams, by which, during the night, a considerable degree of rotation of the eye subsists, in this imperfect sleep; and the subject rises in the morning with all his symptoms increased. During our waking hours, the elevation of the eyelid relieves the eye from the full degree of irritation in its rotatory motion, which it experiences in its movement in the night when closed.

By the means above recommended, all light is excluded, and the eye is preserved in a state of perfect repose, and, of course, is relieved from all irritation.

Brighton, Sept. 1817.

On the Medical Properties of the Phytolacca Decandra. Read before the Linnæan Society of New England, August 30th, 1817. By George Hayward, M. D.

[Communicated for the New England Journal of Medicine, &c.]

A KNOWLEDGE of the medical properties of the indigenous plants of our own country, must be interesting to the naturalists, as well as to the physicians of the United States. In our immediate vicinity, there are several vegetables that have already been introduced into the Materia Medica, and are known to possess considerable power in the treatment of various diseases. Some of our native vegetables may, perhaps, prove valuable substitutes for articles that are now imported from foreign countries, with great difficulty and expense; and, in addition to these advantages, may be always obtained in an unadulterated state.

In investigating the properties of any remedy, however, we are apt to exaggerate its virtues, and conduct our experiments with a view to some preconceived opinions. In this way, a character is given to it, that subsequent experience does not justify, and the medicine is suffered to fall gradually into entire neglect, though it may in reality possess properties of considerable activity. The hemlock (conium maculatum) and fox glove (digitalis purpurea) are vegetables of great value, but they do not enjoy the reputation they would have done, had they not been incautiously extolled as specifics, in certain diseases, in the cure of which they are now known to possess but little efficacy.

In the following experiments, I have been extremely particular in observing the part of the plant employed, and the quantity that was administered, and have, in every instance, carefully noted the effects, though they might have been different from what I had anticipated. These experiments were made in the course of the last eighteen months upon a plant, called by Linnæus, phytolacca decandra. (Synonym. Solanum Racemosum Americanum.) It belongs to the tenth class, and tenth order of his arrangement, and is well known in New England by the names of locum, poke or garget, and is commonly called in Europe the American nightshade. It is a perennial plant, generally found by the side of hedges, on waste land, or near the road. It grows to the height of six or seven feet, with branching, purple stems. Its leaves are alternate, ovate, acute, entire and smooth. The flowers are of a dull white colour, sometimes turgid with red, succeeded by large, round, black berries. It flowers in July and August. It is said to be a native of America, Switzerland, Spain, Portugal and Barbary, though there is some reason, I think, for believing, that it was originally introduced into the old world from this country. This opinion, which is mentioned by Curtis, is favoured by the facts, that the first regular account of its medicinal properties was communicated in a letter from Gov. Colden, of New York, to Linnæus, and that all the knowledge, which the botanical writers of Europe have, at the present day, of its use in medicine, is derived from the practice in this country. It was not introduced into Great Britain, until the year 1640, and even at the present time, it is not employed there, either in medicine or the arts. Several writers have mentioned, that it was used as a medicine by the aborigines of this country, particularly by the Cherokee tribe, as a remedy in syphilitic complaints, though it is probable, that it was of but little efficacy; for Dr. Barton has stated, that this disease continues to commit the greatest ravages among them. We have, however, but little satisfactory information concerning the medical use of this plant by the Indians of our own country; we are made acquainted neither with the part of the plant they employed, the mode in which they administered it, the quantity they gave, nor hardly with the diseases it was supposed to relieve.

The berries of the Phytolacca afford a beautiful purple dye, though no mordant has as yet been discovered that will render it permanent. It is said that they were formerly much used in Portugal to heighten the colour of Port Wine, but this being found to injure the quality, the practice was prohibited by a royal edict.

In this country many people steep the berries in spirit, and this tincture is said to be efficacious in chronic rheumatism. Many birds, particularly Robins (Turdus Migratorius) and Pigeons (Columba Passenger) feed upon the fruit of this plant, and it is hardly to be supposed that it possesses any deleterious properties, as the flesh of these animals is eat freely at that season without producing any injurious effects. In many parts of New England, these berries are known by the name of pigeon berries, as they are the favourite food of that bird. In this section of the country too, the young shoots are boiled and eaten in the same way as asparagus, and they are said to be equally pleasant.

It is frequently mentioned by European writers, that it is a common practice in America to use this plant as a domestic medicine for an emetic, preparing it by putting an ounce of the powdered root to a pint of wine, of which one or two table spoonsfull are to be taken according to circumstances. This practice however I am confident is not general, and the Phytolacca is rarely used in New England as a medicine, except in the treatment of the diseases of domestic animals.

No attempt was made to point out the real value of this plant earlier than the year 1795, when Dr. Shultz published at Philadelphia, a dissertation upon the subject. The information contained in his treatise is not very ample or satisfactory. He administered to dogs a quantity of the expressed juice of the leaves, berries and root, in the dose of an ounce to an ounce and a half, which proved several times emetic and cathartic, "the last operating more powerfully than the others." A quantity of the gummy part of the leaves was given to a dog, which produced some drowsiness and a slight nausea; an equal quantity of the resinous parts proved several times emetic, and "the soporific effects were also much more evident," These were the only experiments of importance that were tried, and the rest of the dissertation consists of remarks on the use of Phytolacca in dying, its chemical analysis, and its efficacy in the treatment of several diseases, particularly cancers, scrophula, syphilis, rheumatism,

&c. though the author seems to have made no experiments himself with it on the human subject.

In the investigation that I have made, I have endeavoured to ascertain whether the Phytolacca should be ranked in the Materia Medica among the emetics and cathartics, what part of the plant should be preferred and in what form and dose it should be exhibited. I have also had an opportunity of noticing its use in some diseases of the skin, in which I think it has oftentimes proved serviceable. I have employed the powdered root and powdered leaves, the tincture, decoction and wine of the root; of the effects of each, I shall speak separately.

Of the Root.

In the first trials which I made with the powdered root, I gave it in doses of five grains, without producing any sensible effect. These were gradually increased to a scruple. Ten grains sometimes produced vomiting, and fifteen usually operated both on the stomach and bowels; though I soon found that I could not calculate with any certainty upon the effects of a less quantity than a scruple, which was administered in every instance with perfect safety. From nearly thirty cases in which I have used the powdered root in the dose of a scruple, I have been led to remark, that that quantity has always operated, except in one case, as an emetic and cathartic, usually three or four times, thoroughly, though never severely; that in general it commences its operation on the stomach in an hour, and rarely continues longer than four. That it excites little or no nausea previous to its operation; and though it makes a powerful impression on the system, it never has produced any disagreeable or unusual symptoms. I have never noticed any dizziness, vertigo or stupor from it, and I have always been particular in my inquiries to ascertain if any such effects took place. I afterwards gave it in the dose of half a drachm, and found that it operated in every instance longer and more powerfully than a scruple. In one case of the commencement of fever, it was administered with decided advantage; it continued to operate for six hours as an emetic, and entirely removed all the febrile symptoms.

The root should be dug late in November, after the leaves and stalks of the plant are killed by the frost. It must then be cut into thin slices, dried with a very moderate heat, and powdered for use.

Of the Leaves.

As the Phytolacca is a perennial plant, it was natural to suppose that the root possessed more active properties than the leaves; and this I ascertained to be true by repeated trials. In one case in which I gave a scruple of the powdered leaves, it produced no effect whatever; in another it operated only once, and then as an emetic; and it rarely had the same effect upon the stomach and bowels as an equal quantity of the powdered root. It should be observed, however, that in one case its effects were extremely troublesome, as it continued to operate both as an emetic and cathartic for more than twelve hours; but these effects were easily removed by a grain of opium. It ought also to be remarked, that this patient was of an unusually irritable habit, and was always powerfully affected by ordinary doses of medicine. In order to know precisely the effects of Phytolacca, I have invariably given it without the addition of any other medicine, and have found, that both the root and the leaves operated more powerfully on the stomach than on the bowels, though a full dose of either rarely failed to act both as an emetic and cathartic.

Of the Tincture of the Root.

An ounce of the powdered root was put to a pound of alkohol, and after standing the ordinary time, was filtered through paper. It was administered to eighteen or twenty patients, at first in doses of half an ounce and then increased to an ounce, without operating as powerfully as a scruple of the root. In one instance the patient complained of feeling very dizzy after taking it, and observed, that there was an unusual sense of tightness about the head. But this was the only case in which I have met with any thing of the kind, though I am satisfied that it not unfrequently takes place, as one or two of my medical friends have noticed it in their practice.

Observing by my own cases, that an ounce of the tincture, which contains two scruples of the root, did not operate so powerfully as one scruple in substance, I concluded, that alkohol did not extract the whole virtues of the plant, and therefore thought it best to try the effects.

Of the Decoction of the Root.

Having drawn off all the tincture, the powdered root was carefully dried, and a pound of distilled water was added to it. They were boiled together for some hours, and then suffered to stand several days. An ounce of this decoction was administered to a patient, and the same quantity was afterwards given to several others, but its operation was rarely so thorough as that produced by the tincture; and after eight or ten trials, I discontinued the use of it entirely.

Of the Wine of the Root.

Knowing that wine would dissolve the gummy as well as the resinous parts of the plant, I thought it would prove a valuable menstruum, and therefore added to a pound of common white wine an ounce of the powdered root. The effects of this preparation however did not equal my expectations, for they were not more powerful than those of an equal dose of the tincture.

An ointment may be prepared by mixing one or two ounces of the powdered root of Phytolacca with a pound of hog's lard, and simmering them together. I have often used this with success in cases of itch, where the ointments of sulphur and mercury have had no effect. It is apt to produce after the first or second application, a great heat and burning over the whole surface on which it is applied; when this is the case, it should be immediately washed off, and I never knew the same effect to follow the renewed application of it. I had the satisfaction of curing a boy with this ointment, who had had tinea capitis for twelve years, which had been treated in different parts of Europe and this country, and had resisted all the ordinary applications. The hair was kept short, the head was washed night and morning, and the ointment was then rubbed over all the parts affected. He never complained of any pain or smarting, though the surface was considerably ulcerated.

The Phytolacca is said to be the basis of a patent medicine for rheumatism, which has obtained some celebrity in this section of the country. I have administered it in the chronic form of this disease, in substance and in tincture, and have found little or no benefit from it. I have not however given it a sufficient trial to offer a decided opinion upon its virtues in this complaint.

Chemical Analysis.

In the 61st Volume of the Annales de Chimie may be found some observations and experiments on the chemical properties of Phytolacca, particularly as applied to the arts, by Mr. H. Braconnot. He first examined the stalks by incineration, and ascertained that a hundred pounds of the ashes yielded forty pounds of pure, caustic potash. The properties of the acid in the stalks of the Phytolacca, agreed better with those of the malic than any other. He ascertained that the colouring matter of the berries, mixed with lime water, was a very delicate test of the presence of acids. He attempted to fix the purple colour on cloths, but did not succeed. He observes that the narcotic virtues ascribed to the Phytolacca are wholly illusory, and that Lemery had discovered the same fact. This remark accords with my experience on the subject.

It was originally my intention to have examined the root of this plant, with a view of ascertaining if it contained gum, resin, fecula and extractive matter; the different proportions of each, and in which the medicinal properties particularly resided. Being unused to analyses of this sort, the results of a number of trials I made were very unsatisfactory. I was not unwilling however to relinquish the subject, when I learnt that Dr. Bigelow proposed to give an analysis of the plant in his work on Medical Botany. From his acquaintance with vegetable chemistry, and the interest he takes in every thing connected with the science of Botany, we have a right to believe, that he will make an accurate examination of its chemical composition. It is understood that the account of Phytolacca will appear in the first volume of his proposed work, accompanied by an elegant coloured engraving.

The use of hot Water to produce Vesication.

[To the Editors of the New England Journal of Medicine, &c.]

Gentlemen,

THAT vesication may be produced almost instantly by hot water applied to the skin, is a fact well known to every one: I would suggest the propriety of using hot water in all cases where it is important to vesicate as speedily as possible—in phrenitis, in hydrocephalus internus, in inflamma-

1st. From the extreme delicacy of that portion of the skin, which is reflected over the eye, giving it a predisposition to be thus affected.

2d. From the saffron hue of the conjunctiva, being one of the first symptoms in icterus.

After the adoption of these sentiments, I superadded to the other means, the frequent use of calomel cathartics, (directing their exhibition every second or third day) and found them fully adequate to the removal of all the symptoms.

I should deem it a matter of some consequence to be able to point out some pathognomic symptoms of this species of opthalmia, by which it might be readily discriminated. The only appearances, which I have found to be peculiar to this affection, are a very troublesome degree of itching, and an extremely irritating discharge from the eye.

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That the principal indication in the treatment of this affection is to restore the tone of the enfeebled vessels, is readily admitted ; and, for this purpose, the whole catalogue of astringents have been recommended and employed.

However plausible the operation of astringents may appear in theory, they frequently disappoint the expectation of the practitioner. In reflecting on this subject, I think more has been expected of them, than is in their power to perform. To produce any important effect, astringents must be long applied to the part, but who does not know, that any extraneous matter, introduced into the eye, is immediately washed out by an increased flow of tears. Doctor Barton, in his lectures, speaking of chronic opthalmy, observed ;—"I know of no remedy, on which much reliance can be placed."

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[Communicated for the New England Journal of Medicine, &c.]

A KNOWLEDGE of the medical properties of the indigenous plants of our own country, must be interesting to the naturalists, as well as to the physicians of the United States. In our immediate vicinity, there are several vegetables that have already been introduced into the Materia Medica, and are known to possess considerable power in the treatment of various diseases. Some of our native vegetables may, perhaps, prove valuable substitutes for articles that are now imported from foreign countries, with great difficulty and expense; and, in addition to these advantages, may be always obtained in an unadulterated state.

In investigating the properties of any remedy, however, we are apt to exaggerate its virtues, and conduct our experiments with a view to some preconceived opinions. In this way, a character is given to it, that subsequent experience does not justify, and the medicine is suffered to fall gradually into entire neglect, though it may in reality possess properties of considerable activity. The hemlock (conium maculatum) and fox was not able to shew the fibrous structure of the white bands of the corpus striatum, although nothing is more easy, if the brain be prepared according to Reil's method; and, when requested to trace them into the grey substance, that he asserted he had "no where professed in his writings, that he could trace the fibres, either of the *diverging* or *converging system*, farther than the bottom of the convolutions," an assertion at complete variance with the passages quoted by Dr. Gordon from his physiognomical work. The inaccuracy of their account of the *second set of diverging fibres* is also pointed out; and the impropriety of regarding as ganglions, the corpus olivare and the optic thalami, are commented on. There is a great deficiency in Dr. Spurzheim's reply to these charges; many of them are scarcely noticed; and a considerable portion of the section is taken up with the account of a dissection of a hydrocephalic brain, in the Edinburgh Infirmary, which was so conducted that neither the pupils, amongst whom Dr. Spurzheim had placed himself, nor the Doctor, could see what was going forward; and the blame of "preventing this case from receiving its proper publicity," is of course ascribed to Dr. Gordon. He is more successful in refuting the charge of plagiarism, and shews pretty clearly that Reil rather owes his views of the subject to Gall, than that the professors have defrauded him. He has also succeeded in refuting the charge, that he had denied "that the white substance, apart from the grey," exhibits a fibrous structure; and asks, "is not the whole order of our converging fibres white?" and also the corpus callosum, the fibrous structure of which he had demonstrated.

With respect to the *converging fibres*, Dr. Gordon proceeds to shew that the statement of the professors, which assumes that, in conjunction with the diverging fibres, these make up the convolutions of the brain, is a mere chimera, and utterly incapable of demonstration; and that their hypothesis of the convolutions being each composed of two laminæ of fibres, joined together in the middle by a *mucous neurilema*, is as unsubstantiated: the experiments attempted in order to demonstrate it being inadequate, while the separation is equally easily effected at any part of the convolutions as at the median line, where the professors assert the layers are united by a *mucous neurilema*, or *very fine cellular tissue*. The fallacy of this hypothesis being proved, that which ascribes to the fluid in hydrocephalus the separation of these laminæ, must appear equally futile, and Dr. Gordon advances sufficient arguments to prove its insufficiency. The separation of the laminæ, he observes, could not be effected without lacerating the substance every where intervening between the ventricles, "and the bottom of the convolutions," which is never remarked to be the case in hydrocephalus, and "could not occur without rupture of vessels and fatal hæmorrhage:" for were the convolutions merely split by the distending powers, they would be found in every state of separation; but long after they become shallow, they are found solid: and he further contends, that the assertion, that "there is no loss of substance in a hydrocephalic brain in any stage of the disorder, is a position not only without proof, but demonstrably incorrect." In answer to these various objections, Dr. Spurzheim maintains that each convolution can be easily separated in the middle line, and unfolded into two layers; but he deserts the position that the laminæ are united by a neurilema; stating that this had never been positively asserted, and that the answer to a query of the French Commissioners, on this point was—"une adhérence de contiguité entretenue *peut-etre* par du tissu cellulaire." It certainly is of little consequence whether the laminæ are thus agglutinated, if they can be unfolded. He still defends the opinions of Gall and himself, regarding the state of the brain in hydrocephalus, and thus explains the changes which take place: "The principal changes take place in the corpus callosum, its appendices, and the convolutions of both hemispheres. The corpus callosum is entire, and lifted up towards the top of the head, the falx is elongated, the convolutions sometimes quite distended like a thin membrane of cerebral substance, from within white and horizontal fibres, and covered on the external surface with cineritious substance. The distention, however, is not mechanical, but also vital and susceptible of modifications, on account of the continual decomposition, and new composition which take place in the organization in general." The accuracy of this explanation has still to be confirmed by future dissections, and accurate examinations of hydrocephalic brains. The remainder of Dr. Gordon's book is occupied with remarks on Dr. Spurzheim's plates, which we do not think of sufficient importance to notice: indeed we would not have commented at all on this controversy, if it had involved merely a question of difference of opinion between the disputants; but it is of the first importance to elicit truth on every subject connected with the animal œconomy, and we know of no method by which it can be so effectually accomplished as by raising doubts, and exciting discussion.

Mr. Charles Bell has thrown out some hints on the structure of the cranium, to shew that its form and joinings prove design, although they have been attributed to accidental circumstances.* He compares the sphenoidal bone to the tie beam of the centering of an arch; intended to counteract what is termed a horizontal thrust, and to prevent the lower part of the parietal bones from being thrust outwards; a security which is chiefly affected by the transverse position of the sphenoid, "and the strong embrace of its alæ upon the edge of the parietal bones." He conceives there is a strict analogy also between "the manner in which the parietal bone sits down upon the temporal bone," and the graining, "which is the foundation of a wall in masonry;" the temporal bones resembling the abutments of the arch of the parietal bones. The skull, therefore, being built as it were on the principles of an arch, we see the reason "why a man falling upon his head," or struck on the vertex, "does not suffer at the part struck, but has a fissure in the temporal angle of the parietal bone." The sutures he regards as joinings by dovetail, or tenon and mortise; and there is evident design in these indentations being on the outer table only of the skull; for as the inner table is more brittle, it "is unfit for this form of union." These illustrations are taken from the work of the mason and carpenter; but, (we think it is an observation of the illustrious Boyle,) "much philosophy is to be acquired in the work-shop of the artizan."

The importance of *morbid anatomy* in elucidating diseases, and thence establishing more successful methods of treating them, becomes every day more obvious, and few practitioners now neglect the opportunity of examinations post mortem. Many very valuable cases of morbid dissection are recorded in the volume of the *Repository* which we just closed.† One of these, detailed by Dr. *Granville*, proves that there was at least some foundation for the statement of several of the older anatomists relative to the existence of a particular duct, for conveying the secreted bile from the liver immediately into the body of the vesicula fellis.‡ Another by Mr. Parkinson demonstrates, in a striking manner, the incorrectness of Dr. Spurzheim's opinion, regarding the integrity of the brain, however much it may be distended, in hydrocephalus, whilst the intellectual faculties remain uninjured.|| Some practical sug-

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* Vide *Surgical Observations*, by Charles Bell, vol. i. p. 478.

† Vide *Repository*, vol. vii. p. 92, 101, 119, 120, 278, 287, 330, 375, 426, 469, 526.

‡ Ibid. p. 158.

|| Ibid. p. 208.

gestions on the treatment of this disease have arisen from the dissection of a case of hernia of the dura mater, detailed by Mr. Earle.* The hernial sac, which was ten times punctured, and a considerable quantity of fluid drawn off from it, was found on dissection to communicate with the ventricles, from which the fluid had been evidently supplied. That the difficulty of lying in a completely horizontal position, on either side, is not always an attendant of hydrothorax, is illustrated by the appearance found on the dissection of a case of dyspnœa, recorded by Dr. *Lettsom* :† a pint of limpid fluid was found in the left sac of the pleura, and two ounces in the pericardium. In the dissection of a fatal case of abscess of the lungs, which had burst externally, and rendered carious the ribs, Dr. *Johnson* found masses of calcareous matter in the abscess, which "appeared like broken pieces of arborescent coral," and "lay partly loose, partly ramified through the other substance." The roots of the pulmonary artery and veins were partially ossified. The liver, which was very large and much diseased, appeared to have been the primary seat of disease.‡ The following remarkable appearances in a case of dropsy were observed in the dissection of the case by *Bezard*. The peritoneum was found to be three lines in thickness, cartilaginous, and resembling at first sight a rind of bacon. The omentum, mesentery, liver and gall bladder, spleen, pancreas, kidneys and urinary bladder, had, as it were, disappeared, and in their stead a scirrhus mass found towards the right side, resembling the liver in shape, and containing pus.|| Besides the instance of complete reversion in the principal organs and viscera, which occurred at one of the dissecting pavilions of the *Hospice de Perfectionnement*, as described by Dr. Granville, in his second letter on the present state and progress of Medical Science in France,§ we have to notice an instance of preternatural position of the heart, related by Dr. Amelung.¶ During the life of the patient the movements of the heart were felt on the right side; and on opening the chest, after death, neither heart nor lungs were found in the left side; but, instead of them, about ten pounds of watery fluid, and a small portion of something like lungs, shrivelled and resembling a dry sponge, the size of the egg of a goose, enclosed in

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* *Medico-Chirurgical Transactions*, vol. iii.

† Vide *Trans. of the Med. Society of London*, vol. i. part ii. art. 9.

‡ Vide *Med. Chirurg. Journ.* vol. iii. p. 255.

|| *Continental Medical Repertory*, No. 1, p. 139.

§ *Repository*, vol. vii. p. 163.

¶ *Continental Med. Rep.* No. 1, p. 137.

a sac of the pleura above the mediastinum. The pleura lining the left cavity of the thorax was thick, like leather, and of a yellow olive colour. The heart was found in the right cavity, with the apex towards the right hypochondrium. "The aorta arose from the anterior portion, but the vena cava had emptied itself into the posterior part. The lungs on this side were tuberculated; and some of the tubercles in a suppurating state. The patient was a tall robust man, thirty-one years of age, and had served five years as a soldier."

With full conviction of the utility of PHYSIOLOGY, we feel much gratification in tracing, not only the rapid progress which is making in that branch of Medical Science, but the prevailing disposition of the inquirers to throw aside those fanciful hypotheses and illusions, which have so long tended to impede the pursuit of truth; and to be guided only by experiment and observation. It is not easy to explain the reason, why Physiology has found so few votaries, comparatively speaking, in this country; while it has found so many, and so much has been effected on the other side of the channel. The most important, and, in fact, the only work generally on this subject, within the last six months, is the second volume of M. Majendie's work, "*Prècis Elèmentaire de Physiologie.*" It contains the history of the *nutritive* and *generative functions;* and is fraught with original and well-conceivèd experiments, admirably qualified to stimulate inquiry in others; but as we mean to lay an early analysis of the volume before our readers, it is unnecessary here to dilate on its merits.

Of a different character, but certainly not devoid of interest, are Dr. Park's Physiological Essays. The continuation of them, which has appeared since the period of our last *Retrospect*, treats of "the laws of muscular motion."* In pursuing his subject, Dr. Park observes, that it is not essential for arriving at correct conclusions regarding the general laws of motion, "that we should ascertain the agent by which muscular contraction is effected, or that of the change it produces;" these laws, like those of matter, being best deduced from the phenomena. On these principles he lays down the following as laws of muscular motion: "1. That exertion is productive of certain changes in the organs of motion, which are accompanied by corresponding changes in the powers of action. 2. That the powers of action have certain limits prescribed to them, which sooner or later require the renovation derived from rest." The continuance of every exertion is followed

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Vide *Journal of Science and the Arts*, vol. ii. p. 224.

by fatigue, and this our author contends, "arises from a change of condition in the moving fibre, effected by the repeated application of the nervous influence:" and that the mind becomes conscious of this change, when it arrives at a certain degree, owing to a portion of nerve being blended with, and entering into the composition of the muscular structure. But he denies "that the sense of fatigue arises from the expenditure or exhaustion of that power or energy imparted by the nerve to the moving fibre; and not from the changes produced by its repeated application. He maintains that the state of the moving organ, on the approach of fatigue, is that of over-contraction, not that of relaxation; and hence nothing is more common than cramps or spasms in the calves of the legs, and the soles of the feet," as fatigue approaches; and when it is carried to excess, there is an actual state of the muscle almost approaching to inflammation, and a rigid contraction, equally in the flexor and extensor muscles. These phenomena are objects of observation and experience in the voluntary organs; but their operation, Dr. Park argues, "can be shewn to extend also to those which are involuntary;" the motions of both being merely "different modifications of the same faculty;" and in both, derived, as Dr. Whytt contended, immediately from the nerves. Notwithstanding the high authorities of Haller and Bichat, who, while they admitted that nerves are the chief instruments of voluntary motions, yet denied that they have any thing to do with that which is involuntary, our author brings forward many and very plausible reasons in favour of the theory he supports. Having established "the unity of the motive power under both its modifications, and the subjection of both to the same general laws," he next examines "the connexion that prevails between the vital properties and the state of the circulation." As our limits prevent us from following his arguments closely, we shall merely observe, on the facts, that those parts of the muscular system most conspicuous for mobility are most abundantly supplied with red blood; and that every change in the state of circulation produces a correspondent change in the degree of mobility; or, whatever increases circulation, and determines red blood to the muscles, is productive of a temporary augmentation of mobility: while, on the contrary, whatever diminishes these, is followed by a diminution of moving power. He endeavours to establish the position, that, increase of mobility uniformly attends increase of circulation, "both in healthy and morbid states; the motive power, as well as the sentient faculty, bearing always, within certain limits, relation to the quantity of arterial blood circulating in the minute or capillary vessels of the muscular system."

In our last *Retrospect** we noticed the very ingenious experiments of Dr. Johnson, confirming the position of Dr. Parry, that "in ordinary states of the system, there is neither dilatation nor contraction in the arteries corresponding with the systole and diastole of the ventricle." Without doing more than merely referring to the controversy between Dr. Johnson and Mr. Hastings on this subject,† in which the objections of the latter were by no means successfully maintained; we have to notice that Dr. Parry's opinion has received a strong confirmation, and has been much elucidated by a fact observed by Professor Dollinger, who was led to conclude, that "by the wave of blood driven into the arteries by the heart, the impulse is communicated to the parietes arteriarum in such a manner, that it may be felt without the artery suffering any dilatation."‡ As connected with the circulation, we may notice here the confirmation of Dr. Gordon's observation, that arterial blood becomes buffy, in a case in which the blood was drawn directly from the temporal artery, by Mr. Dunn of Pickering, and which we have recorded.[*] Having a considerable affinity also with it, is the interesting paper of Dr. J. Davy, which will be found amongst our Selections,[†] on the heat evolved during the coagulation of the blood, in which he supports, from a variety of experiments, the opinion first advanced by Mr. Hunter, "that during the coagulation of blood, there is no sensible evolution of heat."

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* Vide *Repository*, vol. vii. p. 13.

† Vide *Repository*, vol. vii. p. 196 and 364.

‡ *Continental Medical Repertory*, No. 1, p. 128.—The following is the fact from which the Professor drew his conclusions: "Happening to visit some water-works a few years ago, he found that on taking hold of some pretty thick leaden pipes, in which the water was forced up by pumps, he could plainly perceive an impulse, as often as a wave was driven into them, very much resembling that which is felt in the arteries. As the pipes were well secured and fastened, as no trembling could be perceived in them, and as what was felt was the absolute impression of undulation, he could not derive the sensation he felt in his hand from the impulse of the piston of the pump; and thus recollected the pulse, and particularly as in vivisections he had always found it immoveable, he was now anxious to know whether a pulse might be discovered in living animals in an artery when laid bare: he thus laid bare the carotid of a dog, in the presence of several of his pupils, and a pulse was plainly felt in it, though no motion was visible in it. If we now consider, continues he, how seldom actual contractions have been observed in the arteries, we shall hardly be able to persuade ourselves of the vital motions of the parietes arteriarum being the cause of the pulse; and thus many first rate physiologists found themselves compelled to suppose only a mechanical dilatation of the arteries. But nobody, to my knowledge, having ever observed that either, I feel myself now convinced, that the arteries do not suffer any alternate contraction or dilatation at all whilst producing that effect called the pulse."

We would not accuse the Professor of plagiarism; but the following expressions in his reasonings on his experiments, "only the last series of elastic balls is moved, on an impulse being applied to the first, whilst all the intervening ones remain in a state of inactivity," incline us to think he had obtained some information of Dr. Parry's opinions.

No investigations in physiology are of more importance than those connected with vision. Dr. Brewster and Dr. Gordon[‡] have ascertained that the refractive powers of the aqueous and the vitreous humours of the human eye, are greater than that of water, that of the vitreous humour being the highest. These physiologists have also proved, that the crystalline lens exhibits a polarizing structure equal to that of quartz, or the same as the middle of the crystalline in fishes, and the tint polarized by it being a faint blue of the first order. The cornea has an opposite polarizing structure, being nearly the same as that of calcareous spar; or as the outer and inner coats of the crystalline lens in fishes.

No physiological question has elicited so many hypothetical explanations as that regarding the nature of conception and generation; and perhaps on none has so little that is satisfactory been published. Some observations which Sir Everard Home has had an opportunity of making, have led him to hazard a new opinion on the subject. He conceives that impregnation may take place without the existence of the catamenia: and that, whenever any great excitement of the female system occurs, an ovum may pass from the ovarium into the uterus; and that, when it is there brought into contact with the male semen, impregnation takes place.[||] We confess this statement is not very satisfactory to us; but, as a mutilated account only of his paper is before the public, we will reserve our comments upon it until the whole be published. But if the early part of the process of utero-gestation be obscure, much light has been thrown upon its more advanced periods. One curious physiological fact connected with the expulsion of the fœtus, and its spontaneous evolution, we formerly slightly noticed;* but we are now enabled to refer to what we trust will be regarded as a fair exposition of the subject, in our analysis of Drs. Douglas and Kelly's Essays.†

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[*] Vide *Repository*, vol. vii. p. 466.

[†] Ibid. p. 320, and *Journal of Science and the Arts*, vol. ii. p. 246.

[‡] *Journal of Science and the Arts*, vol. iii. p. 134.

[||] Sir Everard was led to these conclusions from the following circumstance:—"A female servant having left her master's house for a few hours, on the 7th of January, and returned in high spirits, was in a few days seized with an epileptic fit and fever, and died on the 15th. On opening the body, an impregnated ovum was found in the uterus, so extremely small that it would have escaped observation, had not Mr. Bauer, by his skill in the use of his powerful microscope, detected and delineated it. It had come from the ovarium of the left side, which was of a larger size than the other.—Vide *Phil. Mag.* vol. lxix. p. 376, and *Annals of Phil.* vol. ix. p. 468.

The most important use of physiological inquiries is undoubtedly the elucidation of PATHOLOGY: and it is in this point of view only in which they can be ever regarded as practically useful. In commencing our remarks on this part of our subject, we cannot avoid noticing the great light that has been of late years thrown upon the treatment of Insanity, by the examination of the state of the brain which professional men now almost generally endeavour to obtain in fatal cases of that disease, or where states of mental faculties approaching to it have existed. In this point of view, we consider Mr. Hastings's account of an anomalous structure of the brain‡ highly interesting. How far, however, the anomalous structure, which was not the effect of disease, and consisted of the deficiency of the posterior cornua of the ventricles, and consequently of the hippo-campus minor, can be regarded as the cause of the defect of intellect of the patient, who was an idiot, it would be rash to decide: but it is singular that, in another head of a man of equally weak intellect, examined by Mr. Hastings, the posterior cornua were also wholly deficient.

The dependance of spasmodic and convulsive affections on some peculiar morbid change in the state of the brain, or some part of the nervous system, has been generally admitted: but the nature of this change has not hitherto been satisfactorily explained. Some new light has been attempted to be thrown on the subject by Dr. Moulson of Chester. He conceives that "when such a column of blood presses upon the brain or spinal marrow, as not entirely to overcome the nervous energy, then convulsions occur." On the same principle he imagines that any irritation applied to a part beset with nerves, will "occasion such a part to exert all its nervous influence to overcome or free itself from such an irritation; of course an increased quantity of blood will be solicited to support these actions; which actions, too long continued, destroying the usual contractile power of the vessel or vessels, will occasion such a debility in them, as to allow of a turgescence of blood here and there, unequally pressing upon the nerves, thereby occasioning spasmodic or convulsive movements."* If this view of the pathology of convulsive action be correct, it is easy to conceive that the greatest benefit must result from the abstraction of blood; and some cases detailed by Dr. Moulson have practically evinced, in this respect, the accuracy of his theory. In a certain degree it is confirmed by the dissection of a fatal case of tetanus by M. Patissier, a French surgeon, who found that "the membranes lining the cavity of the spine," and the internal surface of the heart, "presented a highly inflammatory aspect." †

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* Vide *Repository*, vol. vii. p. 59.
† Ibid. vol. vii. p. 385.
‡ Vide *Repository*, vol. vii. p. 74.

We have heen enabled to lay before our readers a very clear and masterly exposition of "the Primary Elements of Disordered Circulation of the Blood," by Dr. Whitlock Nicholl, which we particularly recommend to the attention of our junior readers.‡ Intimately connected, also, with the pathology of the circulating system, are some very interesting experiments on *the transfusion of blood*, made by Dr. Leacock,§ tending to shew the advantage of this process "in extreme cases of hæmorrhage." Dr. Leacock, reasoning, very justly, that the only stimulus by which the heart and circulating system can be roused when nature is exhausted by excessive hæmorrhage, is the supply of that pabulum which has been lost, found his ideas confirmed by experiments upon animals of the same species. He drained, for example, the blood from a dog, until the animal fainted and appeared at the point of death, and then immediately transfused into its vessels the arterial blood of another dog; the consequence was the restoration of the receiving dog, who, from lying half dead, raised his head; the pulse, before imperceptible, began to be plainly felt; the eye that was dull and glossy regained its former splendour; the respiration became easy and natural; "and the dog, having completely recovered, when let loose, ran about with ease and apparently very little indisposed." From Dr. Leacock's experiments, it appears that the blood to be transfused should, perhaps, be that of an animal of the same species, or nearly so; but it is also evident, from experiments, in which blood from a sheep was transfused into a dog, that benefit may be produced when the blood of a herbivorous animal is transfused into one altogether carnivorous. That this practice may prove useful in excessive hæmorrhages, when blood can be obtained from an healthy individual, is probable; but we are not prepared to accord with the opinion that it will prove useful in diseases connected with deficiency of blood.

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* Vide *Medico-Chirurgical Journ. and Review*, vol. iii. p. 369.

† Vide *Repository*, vol. vii. p. 251.

‡ Vide *Repository*, vol. vii. p. 441.

§ The Inaugural Thesis, containing these experiments, was published last year at Edinburgh. For a plate of Dr. Leacock's apparatus, see *Medico-Chirurgical Journal*, April, 1817

Mr. Salt has investigated the "mode by which constitutional disease is produced by the inoculation of morbid poisons."* He conceives that when a morbid poison is applied to an abraded surface, it is not directly absorbed, but induces a new secretory action in the *very contiguous* arteries, by which a morbid fluid similar to that with which the person was inoculated is generated; and then, but not before, the poison is absorbed and carried into the system. He founds his opinion on the facts that local inflammation always precedes and accompanies the constitutional disease produced; and he believes that, as inoculation of a morbid poison does not always produce this effect, nor is always followed by constitutional disease, the deposited poison, when absorbed without exciting local inflammation and a new secretory process, "is harmless." Hence "excision or destruction of the contiguous organized structure," prior to this process, will prevent the constitutional effects; but these may always be expected to follow, when the local effect is produced and goes through its regular stages. The length of time required for giving the necessary impulse to the secreting vessels, as, for example, in hydrophobia, and the difference displayed in different individuals in this respect, is justly regarded as forming no objection to his theory. The immediate absorption of animal poisons, however, such as that of the rattlesnake, &c. he admits is an exception to the general law.

Connected with the absorbent and exhalent systems, is the following very curious circumstance noticed by Mr. Rigby of Norwich.† A precipitation, if it may be so termed, of sulphuret of mercury occurred all over the skin, giving it at first a dirty, dusky-blue colour, and afterwards a completely black hue, in a patient who had been employing first frictions with sulphur ointment; and soon afterwards, while the fœtor of the skin evinced the continued sulphurous impregnation, began the mercurial friction. The phenomenon has no analogy with the effect sometimes produced by the long continued use of nitrate of silver, and admits of a more easy explanation.

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* *An Essay on the Mode by which Constitutional Disease is produced from the inoculation of Morbid Poisons*, by Charles Salt, 8vo. pp. 88.

† Vide *Retrospect*, vol. vii. p. 266.

It has generally been supposed, that the valve-like obstruction, which the prostate gland occasionally offers to the flow of the urine, is owing to the enlargement of the third lobe: some dissections, however, by Mr. Shaw, have satisfactorily demonstrated that the part projecting into the bladder and forming the valve, is not the third lobe, "but a more anterior part of the gland." He has, also, ascertained "that whenever there is this valvular projection from the prostate, the muscles of the ureters are enlarged," and will be found inserted at the root of the tumour.*

A singular consequence of the existence of worms in the intestines is recorded in a paper of the late Dr. Lettsom. The patient had all the symptoms of enlargement of the mesenteric glands: but after the application of leeches to the abdomen, with soothing fomentations, and the internal use of the pilula hydrargyri, a circumscribed inflamed tumour appeared below the navel, and suppurated. When it was opened, a puff of air escaped; but was not followed by fæces: and a few weeks afterwards, on removing the poultice, a lumbricus, alive, and nine inches in length, was observed in the abscess, and carefully drawn out. Some bubbles of air, fæculent matter, and the seeds of apples and pears which had been eaten, occasionally protruded: but the patient, a female, perfectly recovered.†

The effects of ascending great heights have been often detailed by travellers, but of the histories of this nature on record, few have been so satisfactory as the account of the effects on himself and his party, given by Col. Beaufoy, of a visit to the summit of Mount Blanc.‡ At a height, little above one half of that of the mountain, the appetite for food failed, there was a strong aversion from the taste of spirituous liquors, and the whole party felt extremely drowsy; on still ascending, the thirst became insupportable; and, although the party were continually drinking water, which they carried, yet the urine was not increased in quantity, and the little that passed was very high coloured. "The air itself," to use Col. Beaufoy's strong language, "was thirsty; its extreme dryness," he adds, "had robbed my body of its moisture." When at the base of the small pyramidal mountain, which, correctly speaking, is Mount Blanc, the head became affected with a dull heavy pain, and an acute sensation of pain, also, "very different from that of weariness," was felt in the limbs immediately above the knees; the desire for sleep became irresistible as the party advanced, and the greater part were affected with severe vomitings. Colonel Beaufoy himself was affected with violent palpitations, whilst on the summit;* and his respiration was performed with difficulty; on descending, he experienced, after taking some repose, a tormenting pain in his face, which became one continued blister, and he was unable to open his eyes,† until they had been bathed in warm water for half an hour. Such are the results of the principal investigations on the subjects forming this division of our *Retrospect*, which have occurred within the last six months. If they are few, some of them are not unimportant; and, if properly applied, cannot fail to aid in advancing science.

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* Vide *Retrospect*, vol. vii. p. 423.

† *Trans. of the Medical Society of London*, vol. i. part ii. art. 7.

‡ *Annals, of Philosophy*, vol. ix. p. 97.

Before concluding, we cannot avoid noticing the singularity of three new systems of Nosology having been offered to the profession within a few months of each other. Two of them are professedly mere outlines, and should be regarded as such: but the third is a more complete work, and consequently may be fairly brought to the bar of criticism. In the first,‡ its author, Dr. Parkinson, has attempted the division of diseases into two classes, which he denominates *Hyperbiosis* and *Catobiosis:* in the former, the natural functions are exercised above the limits of health; in the latter, depressed below these limits. Each of these classes he has divided into three orders, founded upon anatomical construction; while he makes the genera dependant upon, and limited to, identity of diseases; the species on situation; and the varities to introduction of poisons. Views so novel necessarily required an appropriate language, and this Dr. Parkinson has invented; but, as has been justly remarked by a cotemporary journal,|| in which the work has been critically noticed, the task he has undertaken is not only arduous and difficult, it "is impossible—to reduce nosology to a progressive, methodical, and logical system, fixed upon solid and just principles, governed by acknowledged and inflexible laws, and divested of fanciful and

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* According to Saussure, the summit of Mount Blanc is 15,700 English feet in height.

† This inconvenience was not experienced by the guides, owing to their wearing masks of black crape.

‡ *Synopsis Nosologiæ, or Outline of a New System of Nosology, &c.* by T. Parkinson, M. D. 8vo. London, 1816.

|| *Edin. Med. and Surg. Journ.* vol. iii. p. 98.

delusive hypothesis." Of the second,* Dr. Nicholl's *Tentamen*, which was given to the public through our pages, and on the character of which our readers must have already decided for themselves, we will not further express an opinion, than that it is highly creditable to its author. Our sentiments regarding the value of the third † are already before the profession. Whatever may be the individual merits of these systems, their appearance is indicative of a desire for the improvement of medical literature, which cannot be contemplated without feelings of satisfaction.

Theory and Practice of Medicine.

Although *Fever* is one of the most common forms of disease in every quarter of the world, and the attention of the physician has necessarily been directed to it from the earliest periods of the history of the healing art, yet the opinions regarding its causes, and in many respects its treatment, are by no means settled. In Great Britain, and wherever British practitioners have been employed, the subject has been closely investigated; and, from observing the almost constant signs of inflammation having existed in some important organ in those cases which have proved fatal, the dangerous doctrines which led to the opinion that debility is the symptom chiefly to be combated in fever, have been fairly driven from the ground they unfortunately too long maintained. It is pleasing to observe, that the same improved mode of reasoning upon the phenomena of febrile affections is beginning to extend on the Continent, and a more efficient practice to be adopted.‡

But it is more especially in the treatment of fevers that modern medicine may exult. We shall notice such improvements as have recently occurred. The management of *Intermittents* is so well understood, that any new proposals regarding them rather tend to excite surprise. The successful revival, however, of an old method of arresting the paroxysms, and, consequently, of removing the disease, will be seen in our translation of Professor Chladni's report of the effect of ligatures to the extremities, in his own case.[*] We leave to the opinions of our readers M. Hufeland's explanation of the fact. The question of the contagious nature of that description of fever, which has been termed "*yellow, or Bulam, or ardent fever,*" is still unsettled. A very able and interesting report, drawn up by Mr. Mortimer of the navy,[†] which, we are informed, "conveys the sentiments of nearly the whole of the medical naval corps," presents many strong facts against the idea of its contagious nature, and refers it altogether to the influence of local causes, such as the effluvia or miasmata, arising from putrifying animal and vegetable matter. This decomposition of vegetable matter he conceives may arise even on ship-board; and he ascribes the rise and great mortality of this fever, as it occurred in the Regalia transport, in 1815, to her being supplied, when on the coast of Africa, with green wood, which he conceives, when acted upon by the bilge water and dirty ballast, generated the fomes of the disease. This opinion is very ably supported by Dr. Dickson,[‡] who, besides, lays much stress upon the disposition of the body of the individual, exposed to the local causes, to receive the disease: and from the youthful, sanguine temperament of the newly arrived European in tropical climates, the system, "goaded by the stimulus of unnatural heat," free living, and exposure or exertion, becomes infinitely more susceptible of fevers, than those assimilated to the climate. Hence Dr. Dickson reckons the tendency to yellow fever to be in the compound ratio of the disposition, and the force of the exciting cause. He considers, that "whatever may be the peculiar coincidence of circumstances, or modification of cause, most fertile in the generation of yellow fever, an uniformly high temperature is the *sine qua non.*" But although it be generally admitted, that a high temperature favours the spontaneous production of yellow fever, yet, M. Moreau de Jonnés [||] has ascertained, that this is not a circumstance essential to its production. Dr. Dickson, anticipating such an objection, observes, that it is incumbent on those who rest their opinions of the contagious nature of the disease, on the fact that this fever does not occur in countries where the heat is more intense, and also in places lying in the same latitude with the Caribbean Archipelago, to shew "why a temperature above a certain height ought to be more favourable to its production; and, secondly, that the climate of two places must be alike, because they lie at the same distance from the equator." He brings forward various authorities to account for these discrepancies, and concludes by remarking, that he is fully convinced that yellow fever arises only in "hot, low, moist, close places, whenever new men are exposed to miasmata, intemperance, or fatigue," and, consequently, that it is not "an imported or contagious disease; but a strictly local and indigenous evil—quod sol atque imbres dederant, quod terra crearat sua sponte." Notwithstanding the extent of the facts and the reasonings which have been brought before the public on this important subject, we must confess that our mind is still unsettled on the point.

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* *Tentamen Nosologicum*, by Whitlock Nicholl, M. D.—vide *Repository*, vol. vii. p. 177.

† *A Physiological System of Nosology; with a corrected and simplified Nomenclature*; by J. Mason Good, F.R.S. 8vo. London, 1817.

‡ In proof of the accuracy of this remark, vide *Examen de la Doctrine Medicale Generalement adopté, et des Systêmes Modernes de Nosologie*; par J. V. Broussais: our analysis of this work, *Repository*, vol. vii. p. 469: *Dictionnaire des Sciences Medicales*, article *Fièvres en particulier*: and *Recueil Général de Medecine*, tome lviii. p. 221. &c. &c.

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[*] *Repository*, vol. vii, p. 253.

[†] *Medico-Chirurg. Journ. and Rev.* vol. iii. p. 9, 93, 182.

[‡] *Edin. Med. and Surg. Journ.* vol. xlix. p. 35,

[||] *Journal de Medicine*, vol. xxxviii. p. 108.

With respect to the susceptibility of the system to a repetition of the fever, we have to remark, that Mr. Mortimer, in the Report alluded to, conceives it can be taken a second time; but the weight of the instances he brings forward * is weakened by the quickness with which these supposed second attacks followed the first; and they have with some reason been regarded rather as "relapses," than second attacks. Dr. Dickson's evidence is, nevertheless, in favour of Mr. Mortimer's opinion; for although indemnity to a considerable extent, may be purchased by a previous attack, "yet," he remarks, "such protection is but relative, and though a sufficient security against ordinary causes is not proof against such as are of great intensity." In a report on this fever by Mr. Comrie, we find the following passage, corroborative of the opinion that the body is susceptible to a second attack: "On our passage to Bermuda, the captain and almost all the officers (of His Majesty's sloop Dasher,) and most of the midshipmen, were again severely attacked with this disease." †

Whatever may be the opinions regarding the contagious or uncontagious nature of this fever, and the susceptibility to a second attack, practitioners, we believe, are pretty generally agreed as to the general principles on which the treatment should be conducted. The practice of phlebotomy in cases of continued fever, which is daily gaining new advocates, has been adopted in yellow fever; and the success of the plan fully warranted its adoption. In the instances which we have particularly noticed, Mr. Mortimer ‡ found bleeding, ad deliquium, the most appropriate, and indeed the only remedy required at the commencement of the attack; but, when the symptoms are indicative of a relapse at this period, he con-

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* Vide *Medico-Chirurg. Journ. and Review*, vol. iii. p. 100.

† *Edin. Med. and Surg. Journ.* vol. l. p. 179.

‡ *Medico-Chirurgical Journ. and Review*, vol. iii. p. 186.

ceives the habit should be prepared by immersing the body in warm water, with a view of exciting the languid circulation, and giving the surface its natural warmth. The bowels should be kept lax by calomel exhibited as a purgative, in which view alone he thinks it useful, and by enemas. To allay the vomiting he recommends effervescing draughts, with carbonate of ammonia and magnesia. Dr. Dickson, also, relies upon the early employment of the lancet, in conjunction with purging and the cold affusion, if indicated: but he prudently fences his recommendation of blood-letting by remarking, that its utility is "entirely dependant, not only on time and quantity, but on the varying state of the animal system; and, consequently, this remedy "is in continual danger of being rated too high or too low." The extraordinary extent, also, to which the bleedings were occasionally carried, must in our opinion have contributed to bring the remedy into some disrepute; for, unless the indications, which could authorize such enormous abstractions of blood as "seventy ounces" at one bleeding, or 250 ounces or more in three days,* were judiciously examined, much mischief might be the result. The free use of the lancet, however, is fully authorized by morbid anatomy. Among other testimonies Mr. Mortimer states, that he has found the meninges of the brain highly vascular, the surface covered with patches of lymph, "and the adhesions remarkably firm;" the stomach also, in some cases, exhibited evident marks of high inflammatory action.

Although we are fully satisfied that too much alarm has been excited on the subject of contagion, and that many unnecessary restraints are often imposed by the quarantine laws, we are by no means prepared to go the length which has been done by Dr. Maclean,† and declare that no fever can be communicated by contagion, and that lazarettos and the quarantine laws should be utterly abolished.

However painful the duty, we feel ourselves called upon to notice the numerous and accumulating failures of cow-pox in preventing small-pox, whether in the natural way, or by inoculation. Our communications on this subject have been numerous; and some of the cases do not appear to have been modified by the previous disease. It is not easy to account for these distressing occurrences; but were we to hazard a conjecture, we would venture to suggest, that it is possible the virus may have become so modified by being confined altogether to the human subject, that its powers of producing the necessary affection of the constitution, which only can be regarded as the test of security, may be so nearly worn out as to be no longer a certain preventive. Hence the necessity of frequently renewing the efficacy of vaccination by procuring the virus directly from its original source. This opinion, however, can be regarded in the light of a conjecture only, calculated to excite inquiry; for the real causes of the fact which it is meant to explain cannot be too minutely investigated.

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* Vide a Paper *On the Ardent Fever of the West Indies.* By Mr. Peter Comrie. *Edin. Med. and Surg. Journ.* vol. I. p. 165.

† Vide *Suggestions for the Prevention and Mitigation of Epidemic and Pestilential Diseases, comprehending the Abolition of Quarantines and Lazarettos, &c.* by Charles Maclean, M.D. 8vo. pp. 108. Lond. 1817.

The methods of treating *erysipelatous inflammation* by incisions in the affected part, suggested by Mr. Copeland Hutchinson,* in that species of the disease denominated *phlegmonodes*, we still regard as a great improvement in those attacks of the disease which are strictly symptomatic of great local irritation.† Dupuytren's practice of applying blisters in similar affections certainly merits, also, attention.‡

The prevalence of *pertussis* has given several opportunities for determining the effects of belladonna in this distressing complaint; and one of the Editors ‖ has had ample proofs of its powerful influence in subduing the spasmodic character of the cough, and shortening the period of the disease. He has generally prescribed, at the same time, confinement to one or two rooms, complete abstinence from animal food, occasional laxatives, and a combination of subcarbonate of soda and ipecacuanha, to allay any disposition to fever, and correct the tendency to acidity of stomach, which the continued use of the ascescent diet recommended induces. The disease in the worst cases, thus treated, has not been protracted beyond five or six weeks; and the hoop has usually terminated in a much shorter period. The convalescence is greatly shortened by the administration of a mixture with decoction of cinchona and soda, and free exposure to the air.

In the method of treating *rheumatism* by percussion and pressure, Dr. Johnson has shewn § that Dr. Balfour has been anticipated by the Oriental practitioners, and, also, by Dr.

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* *Some Practical Observations in Surgery, &c.* By A. Copeland Hutchinson, 8vo. pp. 167, London, 1817.

† *Repository*, vol. vii. p. 219. ‡ Ibid. vol. vii. p. 249.

‖ Mr. A. T. Thomson, who proposes to give the result of his practice, in a considerable number of cases to the public, through the medium of the *Repository*.

§ *Med. Chirurg. Journ. and Review*, vol. iii. p. 109.

Lind of Haslar Hospital, who adopted their method about thirty years ago. Instead of the hand, however, the Orientals use two instruments; one, a wooden cylinder, with a pretty hard, stuffed leather ball on one end; the other, a stick, on which a number of smooth wooden balls are strung, like beads on a wire. The affected part is beaten with the first, then rubbed well with the second, for a quarter of an hour every day, and afterwards bound up lightly in a calico bandage. Dr. Lind found it, on trial, beneficial in many cases of the chronic form of the disease. Dr. Balfour, however, has addressed the Editors of the Medico-Chirurgical Journal, defending his claim to discovery in this practice.*

Several cases of the deleterious effects of the *Eau medicinale* in gout have been recorded by Dr. Bartley of Bristol.†

Some interesting cases of the efficacious effect of large doses of calomel, in combination with opium, in dysentery, have been published by Mr. Power, a naval surgeon.‡ The remedy was given in doses of a scruple, and repeated two or three times a day, until ptyalism came on, after which the violence of the symptoms invariably abated. It ought, however, to be kept in view, that this practice was pursued in a tropical climate. On the importance of an early and free use of the lancet in this disease, we have lately had occasion to express our sentiments; ‖ and we must repeat, that to Dr. Somers § is justly due the merit of having pointed out to modern practitioners the extent to which it may be carried.

We have been rather surprised at an observation of Dr. Woodforde, that practitioners are become skeptical with regard to the utility of blood-letting in *apoplexy*.¶ We believe on few points are they in general so well agreed; and we scarcely know a practitioner who would be deterred by appearances of debility from bleeding freely in any case where cerebral congestion was evident. This effect of plethora has been well illustrated in one of Dr. Leacock's experiments. By transfusing arterial blood from a dog into a cat, until plethora was induced, all the symptoms of direct debility supervened. The contractile power of the heart diminished; the eyes became suffused, slightly protruded, glistening, and excessively red: salivation next ensued, with vomiting, coma, and every symptom of compression of the brain. The animal died in six hours. Dissection displayed symptoms of congestion, both in the brain and abdominal viscera; " a quantity of bloody bile was found in the gall-bladder; and in most of the cavities some bloody serum was effused.*

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* Ibid. No. for April. † *Repository*. vol. vii. p. 275.

‡ *Medico-Chirurgical Journal and Review*, vol. iii. p. 179.

‖ *Repository*, vol. vii. p. 416.

§ *Medical Suggestions for the Treatment of Dysentery, &c.* By C. S. Somers, M. D. 8vo. pp. 78. London. 1816.

¶ *Trans. of the Medical Society of London*, vol. i. Part ii. Art. ii.

We have already noticed Dr. Fouquier's method of treating *paralysis* with nux vomica. We may here mention, that although this physician conceives that almost every case of hemiplegia depends upon an effusion of blood in the brain, yet he does not regard this as an obstacle to the cure, or sufficient to exclude the use of the nux vomica as a remedy; but, at the same time, he justly remarks, that the degree of circumspection necessary in employing general stimulants in hemiplegia, when it is the sequel of apoplexy, or where the mental faculties are much impaired, or relapses demonstrate the disposition to another attack, ought never to be forgotten. He regards the excitement produced by the nux vomica favourable to the re-absorption of the effused blood; and "if," he adds, "as experience has proved, feeling and motion can be restored to the paralysed members, notwithstanding the permanent compression of the medulla spinalis, why should not the same remedy hasten the cure of paralysis, when it depends on a similar cause."† Whatever may be thought of Dr. Fouquier's reasoning, the facts he has detailed are extremely valuable.

Although pathologists have noticed tremours and agitations of the body, as an accompaniment of paralysis, and have employed the term *shaking palsy* to designate some of these, yet the meaning attached to the expression has been vague, and palsy, chiefly characterized by these shakings, has never, until the subject was lately taken up by Mr. Parkinson, been regarded as a distinct disease. The Essay‡ which this author has just published on the subject, is equally remarkable as a specimen of his characteristic modesty, and the acuteness of his observations. He defines the disease in the following words: "Involuntary tremulous motion, with lessened muscular power, in parts not in action, and even when supported; with a propensity to bend the trunk forwards, and to pass from a walking to a running pace; the senses and intellects being uninjured." The tremours commence in the upper extremities, and gradually pass to the lower, until the entire body is so much affected, that the patient is unable to make any progressive movement, even with the assistance of crutches. The tremours, however, occur, although the affected part be supported and unemployed, and are even checked by the adoption of voluntary motion;" but are never accompanied "by a lessened sense of feeling;" while in palsy, consequent to compression of the brain, the diminution of voluntary motion is sudden, and generally accompanied by impaired sensation. The distinction also between the disease our author describes under the term Shaking Palsy, and those passive tremblings to which the name has frequently been applied, and which are chiefly consequent to indulging in spirituous liquors and narcotics, is well defined.

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* For a case analogous to this, in the human body, arising from natural causes, vide *Repository*, vol. vi. p. 116.

† *Bulletin de la Faculté de Medecine de Paris*, &c. No. iv. 1817, p. 359.

‡ *An Essay on the Shaking Palsy.* By James Parkinson, Member of the Roy. Coll. of Surgeons. 8vo. pp. 66. Lond. 1817.

Mr. Parkinson proceeds with some hesitation, in as much as he is unaided by previous inquiries and anatomical investigation, to trace the causes of shaking palsy. The proximate cause he conceives to be, "a diseased state of the *medulla spinalis*, in that part which is contained in the canal formed by the superior cervical vertebra, and extending, as the disease proceeds, to the *medulla oblongata*." Although this opinion must be received with caution, until opportunity enables it to be confirmed by the test of anatomy; yet the progress and nature of the symptoms render it probable. It is only difficult to conceive, that the diseased state should be so local as to exist in the superior part of the medulla without extending to the encephalon, the integrity of which is implied from the absence of any injury to the senses and the intellect. The remote cause is not less objectionable than the proximate; but Mr. Parkinson assumes that it "may be the result of injuries to the medulla itself, or of the theca helping to form the canal in which it is enclosed:" and, reasoning from the circumstances connected with several analogical cases which he details, and the observations of Sir Everard Home on the effects of injury to the *medulla spinalis*, he is led to conclude that these injuries cannot be produced by compression, laceration, or complete division of the medulla; but may be "occasioned by simple inflammation, or rheumatic or scrophulous affection." With regard to the means of cure, every thing which can be expected to accomplish any beneficial effect must be attempted in the first stage of the disease, or while the agitation does not extend beyond the arms. The means suggested are bleeding from the upper part of the neck, followed by vesicatories or issues, calculated to begin and keep up a purulent discharge. No internal remedies are recommended, unless opportunity occur to make trial of mercury: but tonics, and nutritious stimulating diet, are justly reprobated. The slow progress, however, of the disease, and the late period of life at which it occurs, does not allow much to be expected in a remedial point of view.

We have already brought before our readers Dr. Morrison's judicious practice in *Tetanus.*[*] As to the opinion of Dr. Dickson,[†] that every case of the disease is preceded by a torpid state of the bowels, we may observe, that although costiveness frequently ushers in idiopathic tetanus, yet it is seldom a forerunner of the traumatic form of the disease. In the interesting case of the disease drawn up by Mr. North,[‡] the bowels were constipated, and the opium ordered was judiciously combined with purgatives. The benefit which may be reasonably expected from blood-letting in tetanus, is well illustrated by Dr. Macarthur's cases; [||] and besides the illustration of the rationale of this practice, derived from his dissections, the practice gains further support from that of M. Patissier.[§]

It is stated that Brugnatelli has succeeded in curing *Hydrophobia* by means of oxygenated muriatic acid internally and externally employed; [¶] and our correspondent, Dr. Granville, adds four well authenticated cases of perfect cure; and two, in which the development of the disease had been impeded, are recorded.[**] In Calabria, Dr. Scuderi imagines he has effected several cures by means of sulphuric acid.[††] It is unnecessary to make a comment on such statements; the profession will readily appreciate their value.

We may here mention the appearance of a work by M. Villermay, on *Hysterical* and *Hypochondrical* affections.[‡‡] As we have not seen the original, we can merely notice here the opinion of M. Dubuisson, who has reviewed the work in the Journal de Médécine: "It is," says he, "the best and most complete work that has hitherto appeared on these diseases."

In fatal cases of affections of the heart, polypous concretions have been found in the cavities of that viscus, and in the aorta; but it is extremely difficult to determine whether these really existed previous to the death of the patient. In a case of *Enlargement of the Heart*, recorded by Mr. Stewart,[*] dissection demonstrated, that the right auricle, and both ventricles, contained each a large polypus, attached by pedicles, and having so much vascularity and perfect organization, as to leave little doubt that they had existed for a considerable time previous to death.

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[*] *Repository*, vol. vii. p. 306.

[†] Ibid. p. 404. [‡] Ibid. p. 450. [||] Ibid. p. 404. [§] Ibid. p. 251.

[¶] *Philosophical Magazine*, vol. xlix. p. 312.

[**] *Repository*, vol. vii. p. 252. [††] Ibid.

[‡‡] *Traité des Maladies Nerveuses ou Vapeurs, et particulierement de l'Hysteria et de l'Hypochondrie.* Par M. Louyer-Villermay, M.D. &c. 2 vols. 8vo. Paris, 1816.

As if by common consent, medical writers in most parts of Europe appear at length to have become sensible of the neglect with which for ages the disorders affecting the human intellectual faculties have been treated; and hence an infinite number of essays on *insanity* generally, or on particular forms of it, have emanated from the press. Within the period to which our *Retrospect* extends, several have been published respecting the physical and moral condition of the insane, and others are announced as being in progress.

. Mr. Maurice Roubaud-Luce's Recherches[†] bring into a compressed view the observations of ancient and modern practitioners on the subject of that species of insanity which nosologists have defined Melancholia; but the author adds nothing to what was before known regarding this peculiar affection.

Dr. Haslam, who is already so well known by his "Observations on Madness," has offered, in a little Tract,[‡] some very judicious hints relative to the moral management of insane persons; which, if regarded as they merit, would doubtless contribute to the immediate comfort and security, and consequently, although remotely, to the recovery of such patients. But, excepting some remarks on keepers, there is little novelty in Dr. Haslam's Considerations. We cannot but lament, that, on a subject where so much could have been said, and that by an author whose opportunities have been so great and instructive, he has, in this instance, been so succinct as to leave the reader in considerable doubt of the reality of his intentions.

The recent inquiry by a Committee of the House of Commons into the state of madhouses, &c. has exhibited, in a forcible point of view, the necessity of legislative interference and regulation; and hence a Bill was arranged and introduced into Parliament, professing the correction of existing abuses, and the amelioration of the condition of the insane in this kingdom. But the projected measure, when submitted to examination, appeared rather to defeat than promote the humane intentions of the framers; and, by some very extraordinary provisions, to shift the reprobated system of coercion from the patients to their attendants. The errors of the whole of the proposed regulations were also highly injurious to the interests of all those whom they most affected to regard. Dr. Burrows has, in a pamphlet,* exposed these errors, and successfully combated the principles upon which the Bill is founded; and has offered many hints to the consideration of the Legislature upon this delicate and important subject. The effect has been decisive; for the Bill, although not formally disposed of, yet is deemed lost for the present, the second session of its being entertained.

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[*] *Edin. Med. and Surg. Journ.* vol. 1. p. 184.

[†] *Recherches Medico-Philosophiques sur la Mélancolie*, par Maurice Roubaud-Luce, 12mo. à Paris, 1817.

[‡] *Considerations on the Moral Management of Insane Persons*, by John Haslam, M.D. 8vo. London, 1817.

In those acquainted with the speculations of Drs. Gall and Spurzheim, it will not excite surprise to find that either of them should offer their opinions on the subject of insanity. Accordingly, Dr. Spurzheim, has published a volume of Observations.† The lateness of the publication precludes the possibility of our perusing it with deliberation necessary, in fairness to the author or ourselves, to form a decisive opinion of the merits of the work. It is evident, however, from the frequent references of the writer to his Physiognomical System, that this must be considered as an emanation or offspring of that work; without which, in fact, his doctrines and inferences cannot well be comprehended. For the present, as it will speedily come under our review, we shall do little more than give Dr. Spurzheim's division of the contents. After a preface and introduction, the subject is divided into two parts:—1. Derangements of the External Functions of the Mind; 2. Derangements of the Internal Functions of the Mind.—Under the first head he treats of disorders of voluntary motion, and diseases of the five senses: under the second, of diseases of the brain, insanity, its definition, symptoms, division, causes, forms and fits of insanity, prognosis, and the treatment of insanity. The work is embellished with four plates; one of which represents the heads of six idiots, whose brains, with respect to size, were defective in different degrees; another, with the configuration of two skulls of idiots, and the skull of three heads, distended by water in the interior of the brain; the third plate is the plan of an hospital for curable insane patients; and the fourth, the plan of a house for convalescents.

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* *Cursory Remarks on a Bill now in the House of Peers, for Regulating of Madhouses, its probable Influence upon the Physical and Moral Condition of the Insane, and upon the Interests of those concerned in their Care and Management; with Observations on the Defects of the Present System*, by George Man Burrows, M.D. F.L.S. &c. 8vo London, 1817.

† *Observations on the Deranged Manifestations of the Mind, or Insanity*, by J. G. Spurzheim, M.D. royal 8vo. London, 1817.

Within these few days, Dr. Thomas Mayo has also published Remarks on Insanity; [*] but this work we have not yet seen.

On a paper of Mr. Hey of Leeds, containing some facts which he is inclined to think "illustrate the effects of the venereal disease on the fœtus in utero," we have already delivered our opinion.[†] The employment of the nitro-muriatic acid bath in cases of this disease, in which the system appears to have suffered under the continued use of mercury, becomes more general; we have inserted in our pages the manner of using the bath, as described by Dr. Scott himself.[‡]

In closing this portion of our Retrospect it is scarcely requisite to observe, that the catalogue of medical literature has not been very amply swelled within the period it comprehends. We may, however, enumerate the following works on the practice of medicine, in addition to those which we have already more particularly noticed. *A cursory Inquiry into some of the principal Causes of Mortality among Children.* By J. B. Davis, M.D. *Reflections upon Oil of Turpentine; and upon the present State of the Medical Profession in Ireland.* By a Country Practitioner. *Medical and Miscellaneous Observations relative to the West Indies.* By John Williamson, M.D. *Practical Rules for the Management and Medical Treatment of Negro Slaves in the Sugar Colonies.* By a Professional Planter. The second part of Mr. Thomas's *Commentaries on Scirrhus and Cancers. A treatise on the Watery Gripes of Infants.* By Dr. Zugenbuhler.[||] *Memoirs on Chronic Diseases,* &c. By Dr. Berlioz: [§] and *A Medical Account of the Antilles.* By A. Moreau Jonnes.[¶]

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[*] *Remarks on Insanity, tending to illustrate the Physical Symptoms and Medical Treatment of that disease, and founded in the Practice of John Mayo, M.D.* by Thomas Mayo, B.M. &c.

[†] *Repository*, vol. vii. p. 405.

[‡] Ibid. p. 417.

[||] *Uber den Vielfarbigen Durchfall junger Kinder.*

[§] *Memoirs sur les Maladies Chroniques, les evacuations sanguines et l'acupuncture;* par L. V. T. Berlioz, M.D. à la Côte Saint André. 8vo. Paris, 1816.

[¶] *Tableau du climat des Antilles,* &c. Vide Journal de Medecine, tom. xxxviii. p. 85.

As more or less connected also with this part of our subject, we may add *Canine Pathology; or, a full Description of the Diseases of Dogs*, &c. By Delabere Blaine; with a Treatise by M. Dupuy on the *Tuberculous Affection* of some of the lower animals;* and we do not consider, that we lessen, in any respect, the dignity of the profession, by remarking, that much light may occasionly be thrown upon human pathology, by investigations into that of brutes, properly conducted by ingenious and properly instructed inquirers.

Surgery and Midwifery.

We cannot commence our notices on surgery without adverting to the Hunterian Oration, lately delivered by Mr. Norris, the present Master of the College. It is, indeed, a difficult task to infuse much interest or novelty into the descriptions of a traveller, who has to pass over ground that has been trodden and described by hundreds before him; and in a similar degree an annual oration on the same subject scarcely admits even of variety. It is but doing justice, however, to say, that Mr. Norris has displayed, in this performance, considerable skill in the arrangement of his subject, and purity of taste in the style he has adopted; and has produced an elegant specimen of his talents for rhetorical composition.

In estimating the qualifications of a surgeon, too much value has, perhaps, been placed upon skill in operating, and too little on that general knowledge of pathology which is requisite for successfully conducting the subsequent treatment of a case. It is gratifying to see this subject properly treated by such men as Boyer and Delpech,† and not less so by Mr. Bell, in his Surgical Observations.‡ In his third number he brings forward several cases to demonstrate the tendency which "injuries to the frame, whether the effect of wounds or of surgical operations," have to produce disorders of the lungs, by exciting a general high state of irritation. In these instances, which too often prove fatal, dissection exhibits marks of recent inflammation in the lungs, "coagulable lymph exuded, purulency on the surface of the lungs or in the bronchi, and congestion or abscess in their substance." Mr. Bell seems to conceive that this state may be induced independent of any phthisical predisposition in the habit of the patient; an opinion with which we cannot entirely accord; but we fully subscribe to the propriety of his caution, that the "knowledge of this connexion betwixt wounds and the state of respiration, will make us careful to defer all operations when any tendendency to disease occurs." For the cure of this affection, the most active antiphlogistic treatment is requisite; while, at the same time, all local irritative applications should be suspended.

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* *De L'Affection Tuberculeuse, vulgairement appelée Morve, Pulmonie, Gourme, Farcin, Fausse Gourme*, &c. Par M. Dupuy, Médecin-vetérinaire, &c. 8vo. pp. 479. Paris, 1817.

† Vide our Analysis of Delpech's work, *Précis Elementaire des Maladies Réputées Chirurgicales*, 3 vols. 8vo. Paris, 1816: *Repository*, vol vii. p. 120.

‡ *Surg. Observ.* &c. By C. Bell, 8vo. Lond. 1817, p. 241—259.

So much has been lately said and written on the subject of *ligatures* applied on arteries, that it really has become a matter of the first importance, to consider soberly in what the old method is defective, and what the advantages really are which the new methods hold out. On this subject several sensible observations have been made by Mr. C. Bell.* In his opinion, a ligature should be round, strong, and drawn sufficiently firm to indent the coats of the vessel, and interrupt the blood in its course: for if it be so small as to cut the inner coat of the vessel, the outer coat only is left to restrain the bleeding; and if too large, it cannot be drawn sufficiently tight to stop the current of the blood; and a ligature should, in every part of its circle, be in contact with the coat of the vessel; for if other matters be included it is apt to lose its hold. The clot that plugs the artery, and is formed behind the ligature, is longer or shorter, according to the distance of the last branch from the part tied: but besides this clot, coagulable lymph is thrown out from the irritated artery, and is of the greatest consequence in effecting the union of the coats of the vessel, which, from the loss of function, degenerate into a common texture. He advises the taking away the ligatures of the smaller arteries of a stump, at the first dressing, and to twist the ligature on the large vessel, so as to reduce it to the state of a small round cord. This twisting is sufficient to bring away the ligature, which Mr. Bell contends should never be pulled. He is apprehensive the case is not made out, with regard to cutting the ligatures off close by the knot, and healing the flap over them: because the necessity for a change in this part of the operation of amputation is not very obvious, and the foreign body is likely, in the majority of cases, to prove a hurtful irritant. On this important question we would particularly direct the attention of our readers to the experiments and observations* of our valuable friend and correspondent Mr. Cross, whose opinions, in many respects, accord with those we have just detailed. We confess we have, as yet, formed no settled opinion on the subject.

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* *Surg. Observ.* vol. 1, p. 259.

As connected with the subject of tying arteries, we may announce here one of the boldest operations that has ever been undertaken in surgery, the tying the aorta in the living subject. This operation was performed by Mr. Astley Cooper, on a patient at Guy's Hospital, on the 25th of this present month (June.) The man had an aneurism of the common iliac on the left side, which we believe was in that state that life was momentarily in danger. Mr. Cooper immediately cut through the parietes of the abdomen, put aside the intestines, and freeing a portion of the aorta, close to the bifurcation, succeeded in getting a ligature round it. The patient lived thirty-six hours; and on opening the body, no appearance of inflammation, except on a small portion of the omentum, which adhered where the incision had been made, was discovered.

Amputation is often requisite to save the life of an individual when mortification occurs in a limb, which it occasionally does even from slight injuries. The operation, however, should not be hastily undertaken; and the young surgeon would do well to ascertain, as Mr. Bell has remarked in his Observations,† that satisfactory answers be obtained to the following queries, before determining the point: "1. Does the mortification proceed from the violence done to the part, or does it come of the constitution? 2. Has the mortification extended so widely and destroyed so much of the substance, that, in the event of recovery, the limb will be useless? 3. Has a large joint been opened? 4. Has the mortification gone so far, that, even in the event of its stopping, the suppurating surface will be too extensive for the powers of the constitution? 5. Are the great vessels undermined, and is there danger of fatal hæmorrhage?" These questions are not new; but the necessity of attending to them cannot be too often urged. When the operation however is determined on, it should be well done; and in few parts of surgery have so many improvements been introduced. A French soldier, who was wounded at the battle of Waterloo, and whose limb was amputated by Mr. Guthrie at the hip joint, was lately presented to the *Société de Médécine* of Paris, by M. Larrey, as another proof of the success of this operation.* Every praise is undoubtedly due to M. Larrey, for his exertions in removing the prejudices against the hip operation by his example; and we are not unwilling to grant him the honour he claims, of having first practically demonstrated its safety and propriety.

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* *Repository*, vol. vii. p. 353.

† *Surgical Observations*, vol. i. p. 303.

Previously to performing another important operation also, *lithotomy*, it is of much importance to determine, whether the stone be free in the bladder, or sacculated. In the work† we have had occasion already to quote, the following rules for this purpose are laid down. When the stone does not change its place, nor stop the flow of the urine; "when in sounding, it is felt always at the same point; when that point retires if the bladder be full, and approaches if it be empty; when the sound cannot be got round the stone, but only to touch it, we must doubt whether the stone be free in the bladder." If the stone be sacculated in the coats of the bladder, at or near the fundus, Mr. Bell recommends the high operation; "and in performing it, to lower the handle of the sound until the part of it in the bladder and in the sac, be felt through the inner coats of the cell." This is then to be cut open with a sharp bistoury; the wound enlarged with a probe-pointed bistoury, and the stone seized. To insure the success of the operation, a catheter should be kept in the bladder. If the stone be fully ascertained to be sacculated in the neck of the bladder, by the usual first incision carried directly upon the face of the calculus, "it might be extracted without cutting into the bladder at all; and if the sacculation be behind the muscles of the ureters, it is possible to cut upon the gripe."

Some remarks on the morbid growth of the cornea, which gives it a conical form, with a new method of treating it, have been published by Sir Wm. Adams.‡ This state occurs without inflammation, is progressive, and the thickness producing the conical form, is chiefly in the centre. The cornea, when examined in front, sparkles with a brilliancy like crystal; a circumstance, which Dr. Léveille attributes to the cornea reflecting, instead of transmitting, an opinion which Sir William's operations have proved to be well founded. The vision, if not utterly destroyed, is so indistinct, as to oblige the patient to be nearly as dependant as if totally blind. From a case which occurred, in which cataracts were combined with conical cornea, and in which, after the removal of the cataracts, the patient could see more distinctly without convex glass than is usual after the operation, and with a convex glass small print could be read without difficulty, Sir William was induced to try the removal of a healthy crystalline "as a remedy for blindness produced by conical cornea;" and the result was perfectly satisfactory. When the crystalline is healthy, or in a state capable of being broken down, he properly recommends it to be removed by that method as preferable to extraction.

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* *Bulletin de la Faculté de Medecine de Paris*, 1817, p. 316.

† *Surgical Observations*, vol. i. p. 447—460.

‡ *Phil. Mag.* xlix. p. 110.

Dr. Lobenstein Lobel has proposed to extract the diseased lens in cataract, through the conjunctiva instead of the cornea.* He conceives, much benefit is likely to result from this, "when the capsule, half or a whole line thick, is morbidly indurated, or when the lens is very large and cannot be broken down," by its setting aside any chance of prolapsus of the iris; and by the reunion of the divided parts being more expeditious than on making the incision through the cornea. We may remark as an objection to this proposal, that wounds in, the conjunctiva are followed by more inflammation than those in the cornea.

On the subject of *fungus hœmatodes*, or soft cancer, some very valuable cases and remarks have appeared.† Mr. Bell is of opinion, that the only remedy, is recourse to the knife the moment the disease is ascertained; and if it have "got entangled in the vessels and muscles of a limb," nothing offers hope but amputation; but even this will not avail, if the muscles "exhibit a pale, blanched, and fishy appearance." Another set of tumours, nearly as dangerous if neglected as the soft cancer, we mean those which rise from the gums, have also engaged the attention of this excellent surgeon. A tumour of this description first appears as "a small hard prominence of the gum, shooting out betwixt two teeth." If those be not decayed, we may conclude, that the cause is deep, and not to be removed by pulling the teeth; but if they be bad, there is a reasonable hope of arresting the progress of the disease, by removing them. If the tumour originate "in the membrane of the fang, or in the socket," then the "whole system of parts, the whole of what is connected in constitution, must be removed." These opinions are illustrated by several very interesting cases.[*]

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* *Edin. Med. and Surg. Journ.* I. p. 59.

† *Surgical Obsarvations*, vol. i. p. 365.

When *hernia* occurs in early infancy, it is generally of that description which has been denominated congenital; in which the protruded bowels are found in contact with the testicle; but instances have occurred to prove, that real hernia may occur at a very early period; in testimony of which, the dissection of a case has been published by Mr. Howship, in which the hernia had no connexion "with the tunica vaginalis testis."[†]

We notice as a valuable addition to works on hernia, the dissertation of Dr. Hesselbach, which has been lately translated into Latin by Dr. Ruland.[‡]

Connected with this subject, is the operation on the artificial anus at the groin, suggested by M. Dupuytren, and described in our last *Retrospect*. We had much satisfaction in being able to lay the plates illustrative of this operation, before our readers; and shall seize the first moment of putting them in possession of M. Dupuytren's description of its various steps, as soon as it is given to the public.

No material changes have taken place in the practice of Midwifery within the period of our present *Retrospect*, if we except the invention of an improvement in the craniotomy forceps, by Dr. Davies, Physician to the Queen's Lying-in Hospital; the advantages of which the following description will render evident. The instrument is intended to complete the delivery speedily and safely, in those unfortunate cases, where it is previously necessary to diminish the bulk of the child's head. No other instrument has been proposed to answer this intention, either in ancient or in modern times, that is not liable to very strong objections. The order of laborious labours requiring the use of instruments of this class, has been therefore regarded as embracing the least cultivated department of instrumental midwifery.

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[*] *Surg. Obs.* vol. i. p. 413-28.

[†] *Med. Chir. Journ, &c.* vol. iii. p. 179.

[‡] *F. C. Heeselbach, M.D. in Theatro Anatomica Wirceburgensi Prosectoris, Soc. Med. Erlang. Membri, Disquisitiones Anatomico Pathologicæ de Ortu et progressu Herniarum inguinalium et cruralium, accedit Descriptio instrumenti, hæmorrhagiis sub herniotomia ortis et secure detegendis, et sistendis apti, iconibus illustrata.* Latinitate donavit Th. Aug. Ruland, M.D. &c. 4to. p. 80. Wirceburgi, 1816.

The instrument, now proposed, consists of two handles and two blades, with a locking part common to each, to be adjusted precisely in the same manner as those of the ordinary midwifery forceps. The handles, as far as the lock inclusive, are in no respect different from those of the common forceps. The blades, however, are not divergent from each other, but are produced in a parallel direction, remaining very near each other, until they meet at their expanded extremities within about an inch and a half of their terminations. At this part, the opposite blades are made to embrace each other so firmly, as to have the power of detaining within their grasp substances of the utmost tenacity. Both blades are gently curved, to adapt them to the axis of the pelvis and to the shape of the child's head. Each blade is to be introduced singly. The external blade is to be insinuated on the outside of the head, which, in most cases, cannot be very difficult, after a proper quantity of brain has been removed. The internal blade is to be introduced into the inside of the head, through the perforation which has been already made on it by the operation of craniotomy. The corresponding parts of the instrument are then to be brought together by a careful adjustment at the lock. This adjustment will have the effect of causing to be embraced between the blades, a portion of the child's skull on the inside, and of the integuments covering that part of the skull on the outside, corresponding to the dimensions of their grasping extremities. The internal surface of the external blade, it will now be readily inferred, is concave, to adapt it to the convexity of the child's head on the outside. On the contrary, the internal surface, by which is meant the grasping surface of the internal blade, is *convex*, that it may come in contact more readily, and at many points with the concave surface of the child's skull interiorly. But as we operate in cases of this kind in circumstances often of great confinement or distortion, much force of traction must in many instances be employed, and therefore without great tenacity of purchase, any instrument would be liable to slip. This accident has been provided against in the following manner. The expanded extremity of the internal surface of the external blade is hollowed out into a receptacle like that of a spoon. Attached to, and rising from the bottom of this receptacle, are a number of sharp teeth, the points of which are under the level of the brim; and are therefore secure from the reach of any part of the mother during the introduction of the external blade. Corresponding with these teeth of the external blade, imperforations are drilled into the convex or grasping surface of the internal blade. Upon the handles of the instrument being forcibly brought together, the adjustment at the locking part having been propely attended to, it is manifest that the imperforated convex surface of the internal blade must have the effect of pressing the embraced part of the skull and integuments of the child's head upon the sharp teeth of the opposite part of the instrument; or, in other words, that the teeth of the external blade will be made to penetrate the integuments and skull, at every point of surface exposed to their action.

In cases of great distortion, it will sometimes happen, that portions of the frontal, temporal, and parietal bones, may be removed in fragments of greater or less magnitude; while the occipital part of the head shall remain fixed above the brim of the pelvis, hitching over the pubis, and beyond the safe reach of the instrument which has been described. It has, therefore, been found expedient, to apply the same principle of purchase to cases of this class in another form of the instrument. The difficulty of applying the first form of the instrument to this description of cases, arises from the direction of its curve in relation to its handles, it being impossible to introduce the external blade immediately behind the symphysis pubis, so as to embrace the occipital portion of the child's head resting there, without producing dangerous pressure with the handle upon the perineum and os coccygis.

The instrument of the second form is therefore so constructed, as to obviate this difficulty. The blade part is precisely the same with that part of the first; (here is to be understood the whole of the curved part of the first instrument;) but at the termination of the curve, where each handle and blade may be supposed to meet, (allotting to the handle a much larger portion of either half of the instrument than is covered with wood,) the handles of the second instrument are made to bend considerably backwards in the direction of the convex surface of the external blade; so as to enable the practitioner to introduce both blades of his instrument perfectly and safely, and embrace the occipital part of the child's head, which is supposed to be wedged in a state of arrest, immediately behind and above the pubis, without the necessity of disturbing the perineum and the parts adjacent with the handle of either part of the forceps. This form of the instrument may be called the doubled curved craniotomy forceps. In a very large majority of unfortunate cases requiring the operation of craniotomy, the delivery will be satisfactorily completed by the abundant power over the form of the head and the resistance made to its descent by the first firm grasp and steady pull of the instrument which has been first described.

The external part of each instrument, measured singly, is considerably longer than its corresponding or antagonizing internal part. In the instrument with the single curve, this is more remarkably the case than in the other, arising from the circumstance of its describing necessarily a segment of a larger circle than its antagonist.

It is intended here, however, to request attention more particularly to the extremity of the grasping part of each half of the instrument. The termination of the external part of each form of the instrument is lengthened out about one half or three quarters of an inch beyond the brim anteriorly of its receptacle in the blade, and therefore so much beyond the extreme point of the convexity of the internal part of the instrument.

Persons, who are familiar with cases of distortion, will be able to attest the correctness of the remark, that what is called obliteration of the orifice of the uterus, would never take place in many instances for want of the ordinary pressure upon it from above; there being no room above the pelvis to put it upon the full stretch of distention. In a proportion therefore of such melancholy cases, a part of the unobliterated lip or orifice of the uterus will be found hanging down loose and pendulous into the vagina. Now the extremity of the external part of the instrument is lengthened out, as described, beyond its internal antagonist, for the purpose of carrying before it, and by that means of removing beyond the purchase of its own grasp, any unobliterated part of the os uteri which may present itself during the operation.*

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* Dr. Davies was presented with the gold medal of the Society of Arts, for this invention. It may be useful to mention that the person who made Dr. Davies's instrument, is M. T. Batscham, 35, Worship-street Finsbury-square.

REVIEW.

Commentatio de Pathologia Lienis, Observationibus per Anatomen institutis, indagata, ad illustrandam Physiologiam ænigmatici hujus visceris. Auctore C. L. Schmidt. *Gottingae.* 1816,

THE object of this dissertation is to illustrate the physiology of the spleen, by observations relating to its structure and diseases. It commences with an exact description of the situation, form, magnitude, and weight of this organ; of its lymphatic vessels, nerves, and especially of its great artery and vein.

The diseases of the spleen have been neglected or mistaken, from being confounded with those of other parts. The vomiting of blood, dyspepsia, difficulty of breathing, have been considered as symptoms of different diseases; and the unfrequency of a fatal termination has prevented the truth from being discovered after death. Yet the derangements of the spleen are so common, that it is more rare to find it in a sound than in a diseased state. The inflammations of this organ are divided into *acute* and *chronic;* the acute inflammation varying as to its extent, through the whole, or only to a portion, is either *universal* or *partial:* and as other viscera are occasionally inflamed at the same time, it is also to be considered as *simple* or *complicated.*

As the inflammation of an abdominal organ is rarely confined to that organ during its whole course, it will be difficult to distinguish the acute inflammation of the spleen from that of other parts, except in the earliest stage. As to the predisposition, it appears that persons of both sexes and of all ages are liable to the complaint, but it most frequently occurs in females, who are affected with a deficiency or entire suppression of the menstrual evacuation; that the melancholic temperament is more disposed to it than the sanguine; and that it is observed most frequently in humid and warm countries, such as Hungary, Bengal, and Italy.

The occasional causes are such as the application of cold to the feet or breast, a concussion, a blow, a fall, suppression of the menstrual or hæmorrhoidal fluxes, or of any other customary discharge of blood.

Inflammation of the spleen is preceded by the following symptoms: The patient becomes sad, morose, and inclined to anger; there is a sense of uneasiness in the præcordium, an aversion to food, and a difficulty in digesting it; the countenance becomes pale, livid, and often, from the sympathetic affection of the liver, of a yellow colour. To these symptoms are added chills, followed by heat; the patient is uneasy and cannot rest either on the right or on the left side, but is most easy in a supine posture, and yet unable to sleep. In consequence of the little irritability of the spleen, these symptoms are protracted to the fourth, and even to the seventh day, which is not observed to happen with the other abdominal viscera.

Symptoms of the disease.—1. The most constant symptom, and one which leads with most certainty to distinguish the disorder of this organ, is an *anxiety and straitness in the præcordium,* with difficult respiration, often conjoined with a cough without expectoration. This may be supposed to arise from a sympathy of the lungs, rather than inflammation of the diaphragm, or pressure from the enlarged spleen. 2. The patient complains also of internal *heat, tension and pains* in the left side, which sometimes extend through the whole region of the abdomen, or shoot through the diaphragm, and into the left shoulder: these pains are increased by pressure, and are pulsatory, pungent, and burning in various degrees. 3. *Pulsation* and *palpitation* in the left hypochondrium and back. These symptoms are sometimes violent. Tulpius relates a case " where he could distinctly hear the stroke of the beating spleen, at the distance of thirty feet." 4. The pulse in the left side is sometimes suppressed, often intermittent, weak, and not quick. 5. *Lassitude* and loss of strength. 6. *Watchfulness,* with delirium. 7. *Dyspepsia, anorexia, vomiting* of green bilious matter, and sometimes difficulty of urine from affection of the kidney or bladder. 8. *Swelling* in the region affected, representing the form of the spleen. 9. *Fainting.* 10. *Bleeding* from the nostrils, at the height of the disease, which, in due quantity, terminates the complaint; and the more profuse it is, the more does it alleviate the fever, pain, and vomiting of blood. Sometimes, however, it is so excessive as to prove fatal. It is wonderful, that from Hippocrates to the present day, this should be observed to occur from the left nostril. 11. The most remarkable symptom attending this disease is the *bloody vomiting,* which most authors have considered as peculiar, and have designated by various names. Hippocrates called it melæna. Schenkius, dejectiones nigræ. Guaronius, morbus niger. The ancients, bilis atra; for they supposed it to consist of true bile. Marcus was the first physician who gave sufficient weight to this symptom; he even says that every bloody vomiting depends on the spleen; and that an inflammation of this organ does not occur without it. But surely, if this phenomenon is so constant, we ought to meet with it more frequently, since the spleen is so often disordered.

The usual course of the complaint is this: The patient, after being unwell some days and vomiting mucus, the gastric fluid and bile, is attacked with fainting and vomiting of bloody, frothy, and sometimes black coloured liquids, and soon after the intestines, which were previously confined, become relaxed, and emit substances coloured by black blood, which is sometimes thought to proceed from a hæmorrhoidal complaint. The vomiting of blood is, under common circumstances, to be considered a favourable and critical evacuation; yet sometimes it proceeds to a fatal excess.

Dissections of dead bodies shew that the spleen is inflamed, and even gangrenous; and the surrounding organs partake of the inflammation.

The author afterwards treats of the slow inflammation of the spleen, of its displacement, change of texture, deficiency, extirpation, and rupture.

The conclusions drawn from a multitude of anatomical and pathological facts are these: that the spleen is an organ peculiar to red-blooded animals; that it is of the greatest importance in preparing and mixing the blood; and that its action is of as great importance to the liver, as that of the lungs to the heart.

In support of which opinions, the following arguments are summarily adduced:

1. The singular influence of the spleen on the blood is shewn,

a. By the consent (sympathy) observed with the liver, heart, and lungs, in diseases of the spleen.

b. By the increased quantity of blood in the whole body, when the bulk of the spleen is increased.

c. The many secondary affections of the system, conjoined with diseases of the spleen.

f. The experiments of Home exhibit the singular influence of the spleen on the blood; since heterogeneous particles, as, for example, of rhubarb, were separated by the assistance of the spleen. Hence a dispute, whether the spleen contributes to the formation of bile; which seems probable, since it appeared, in the dissection of animals who had suffered extirpation of the spleen, that the bile was colourless.

2. The business of the spleen, as a venous lung, is demonstrated by the following facts,

a. The operation of diseases on the spleen, as on a vein; for example, its enlargement, ossification, cartilaginescence.

b. The change of colour in the spleen with advancing years.

c. The increase of the venous system, observed on the increase of the size of the spleen.

d. The great consent of the spleen with the lungs.

e. The greater size of the spleen in animals inhabiting marshy regions.

f. The structure of the spleen being of a venous character.

g. Glands in the spleen, which may be compared to the bronchial glands.

i. Enlargement of the spleen, often observed before the menstrual evacuation.

Whatever may be thought of the author's deductions, we must give him credit for contributing to the improvement of medical science, by assembling and arranging a multitude of facts relating to the structure and diseases of this *enigmatical viscus.*

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1. *A Dissertation on the Mineral Waters of Saratoga, including an Account of the Waters of Ballston. By* Valentine Seaman, M. D. *New York*, pp. 131. 1809.
2. *Analysis of Ballston Water, communicated by Dr.* Hosack *to Dr.* Miller. *New York Med. Repos. Hex. 2d, vol.* v. 1808.
3. *Observations and Experiments on several Mineral Waters, in the State of New York. By Mr.* John Griscom. *Bruce's Min. Jour.* vol. 1, p. 156.
4. *Chemical Examination of the Water of the Congress Spring, Saratoga. By* J. F. Dana, M. D. *New Eng. Jour. Med.* vol. vi. p. 19.
5. *An Experimental Inquiry into the Chemical Properties and Medicinal Qualities of the principal Mineral Waters of Ballston and Saratoga, in the State of New York, &c. By* William Meade, M. D. *Mem. of the Am. Phil. Society of Philadelphia, &c. &c.* 8vo. pp. 167. *Philadelphia,* 1817.

6. *Analysis of the Mineral Waters of Ballston and Saratoga, &c. &c. By Dr.* John H. Steel, 18mo. *Albany,* 1817.

BALLSTON and Saratoga are now become such fashionable places of resort during the summer season, either for amusement or the restoration of health, that the public at large feel some curiosity to know the circumstances, which are thus sufficiently powerful to draw together such numbers of visitors from all parts of the United States. In order to gratify, so far as is in our power, so laudable a wish, we have collected all the publications which have come to our knowledge, and from these we shall endeavour to present to the reader, an account of the country in which these springs are found, of the nature of the substances contained in their waters, and of the uses to which they have been applied.

The village of Ballston is situated in the county of Saratoga, state of New York, about two hundred miles north of the city, and twelve miles west of the Hudson river; it is in a deep valley, surrounded by a range of undulating hills, which form a kind of amphitheatre, and is intersected by a branch of the river Kaydarosoras. The soil is naturally poor and sandy, bearing oak, pine, and hemlock; but in the vicinity of the village it has been rendered more productive by the use of plaster of Paris. The ground is principally composed, says Dr. Meade, of two or three species of rocks, of secondary formation, but these are so covered with immense beds of sand, that it is difficult to ascertain this formation, and it can only be done by an attentive examination of the rivulets, which in some places have laid bare the strata. Large blocks of quartz, and rolled masses of primitive rocks, are distributed over the surface of the ground in and about the village, which circumstance perhaps gave rise to the opinion of Drs. Seaman and Mr. Griscom, that there are many rocks at Saratoga, &c. approaching in hardness to porphyry; but we believe, with Dr. Meade, that these are to be considered as accidental, and by no means to determine in any degree the geological character of the place. Mr. Griscom affirms that neither calcareous nor magnesian stones are to be found in the neighbourhood; but Dr. Meade, who appears to have studied the structure with much attention, has discovered, that where the stratum of soil or sand is washed away, an entire range of flœtz, or horizontal rocks is laid open to view, consisting of a species of schist which is nearly black, which stains the fingers and effervesces slightly with acids. It appears to contain both carbon and carbonate of lime, and to approach in character the aluminous schale. It breaks into columnar or prismatic masses, and its fractured surfaces present the impressions of a species of grass. Within three miles of Ballston, solid masses of calcareous rock may in some places be observed, particularly near the site of a mill, where the flœtz formation is beautifully illustrated. This limestone is nearly of a black colour; its fracture is slaty; it abounds with shells of various forms, some of which are so very apparent in their structure and form as not to be mistaken; they principally consist of bivalves, madrepori, terebratulites, corrolites, and echinites; so exceedingly similar, says Dr. Meade, to fossils in my possession from Mendip, in England, that it is difficult to distinguish the specimens from each other. When rubbed, this stone emits the odour of the *pierre puante.* The rocks of Saratoga present the same general characters, but the shells are not so abundant, and they are penetrated by veins or seams of flint or chert. These observations of Dr. Meade appear unequivocally to prove the geological character of the vicinity of the springs. According to Dr. Steel, sulphuret of iron, plumbago, and small specimens of fluor spar are found in the neighbourhood.

These villages owe their celebrity to the mineral waters, which are poured forth most copiously from many parts of their surface. It appears from Dr. Steel's work, that they had been known, from time immemorial, to the aborigines of the country, whose attention, according to their tradition, was first attracted towards them by the great quantity of game by which they were frequented; and this notice was afterwards kept alive by the salutary iufluence of the waters upon some of their diseases. In the year 1767, Sir William Johnson, who suffered from the gout, was persuaded, through the recommendation of the Indians, to visit the spot and drink the waters from the fountain. He passed through the wilderness, remained some time in the neighbourhood of the spring, and returned renovated by health. Some time after Sir William's return, a man by the name of Norton, influenced by the growing celebrity of the spring, obtained permission, from the owners of the soil, to erect buildings and clear the land in the vicinity. Under this permission a small hut was built, and a clearing made; but the fear which the hostile Indians inspired, during the revolutionary war, which commenced about this time, induced him to abandon his improvements. After the contest was decided, Norton returned to his old abode, and farther settlements were made in 1784 and 1785, by others, by whom the three springs, now known by the names of Flat

Rock, President, and Red Spring, were probably discovered. The discovery of similar springs at Ballston, about the year 1787, checked the growth of the settlement at Saratoga, and nothing of consequence was done until the year 1803, when the valuable qualities of the Congress Spring having been duly appreciated, Mr. Putnam was induced to erect a large house for the convenience of those who were allured thither to drink the waters. Since that time, says Dr. Steel, the waters of Saratoga have gradually gained a celebrity that appears as firmly as it is justly established. Numerous houses for the reception of company have been erected, and the influx of persons the present season, from all quarters of the Union, has, we understand, much exceeded the amount of any former year.

At Ballston there are three springs, which have gained notoriety. The first, or the Public Well, so called from having been reserved for the benevolent purpose of serving the public by Sir W. Johnson, in the original grant of the land to private individuals, is situated nearly in the centre of the village. This spring, says Dr. Meade, issues from a bed of stiff blue clay and gravel, which lies near a stratum of schist or shale, nearly on a level with the brook or rivulet, which runs through the town, and the course of which has been diverted from the springs by a dyke or mound. A circular vessel of wood forms the well, in which the water rises to within four feet of the surface, and is dipped out in glass tumblers fixed to rods, a method which is certainly inconvenient and awkward, and which might be easily remedied. The inhabitants have surrounded the well with a marble pavement, supporting an iron balustrade.

The second spring rises about two hundred yards from the public well, and is the property of an individual. It is situated lower in the valley, not many feet above the level of the rivulet; the ground around it is not high, and it is covered with a building, which contains conveniencies for warm bathing.

The third which, from the presence of sulphuretted hydrogen, has acquired the name of the Sulphur Spring, rises within twenty feet of the one last described.

By digging in the neighbourhood of the two last wells in almost every direction, springs will be found, says Dr. Meade, exhibiting the same qualities in a slight degree, but by no means so strong as the waters of Lowe's spring, which, with the public well already mentioned, are the principal ones that are generally and indiscriminately used. " The situation of the country round Saratoga differs but little from that of Ballston, except that the hills are not so high, and the valley is more extensive. It lies low, and the soil is principally sand, or gravel, covered with peat. In this valley a number of springs are to be found, showing more or less the same sensible qualities. To these wells different names are given, such as the Congress Spring, the Flat Rock, the Hamilton, the President, the Columbian, the Round Rock, &c. They are but a short distance from each other; and, as the same appearances present themselves in every part of this valley, many more may certainly be found if it was examined."

"The water in each of those springs arises from a bed of sand, intermixed with stiff blue clay, and overlaying the calcareous and schistose rocks.

"The source of these springs does not appear to lie deep, as they are all found within five or six feet of the surface."

"They are all confined or enclosed in circular or square wooden vessels, not more than five or six feet deep."

"One of these springs, called the Round Rock, has something so peculiar in appearance as to attract particular notice. It is situated at the foot of a calcareous rock, low in the valley, and is covered by a cone or pyramid near six feet high; this cone is hollow, and has an opening at top about nine inches wide, from which the water can be seen in a state of agitation, as if boiling, from the extrication of gas, which rises to the surface. An opening at the bottom of this cone, about four or five inches wide, on a level with the surface of the ground, gives exit at present to the water." This cone is supposed by Dr. Meade, and we presume correctly, to have been formed by the gradual deposition of the carbonate of lime, which this water is known to contain. pp. 19 and 20.

The waters of all these springs, except the one which is impregnated with sulphuretted hydrogen, coincide in their characters, differing from each other only in degree. When viewed at the wells, the surface appears in continual agitation, and a vast number of small air bubbles are seen to rise and break. Small animals, immersed in the air of the wells, are soon suffocated. When first taken from the springs, they are perfectly transparent and colourless; they emit a number of air bubbles, sparkle when poured from one vessel to another, and their taste is first pungent, afterwards saline, and finally ferruginous; on being allowed to remain at rest for a few minutes, especially in warm weather, the gas which they contain separates, and covers the whole internal surface of the glass with minute bubbles; they then soon lose their transparency, a pellicle, slightly iridescent, appears upon the surface; a cloudiness, or opacity, ensues, and finally there takes place a precipitate of a light brown powder, which adheres firmly to the glass. After this process the water resumes its transparency, but its taste is saline and disagreeable. Such are the sensible qualities of the Ballston and Saratoga waters. We shall now present the reader with the results of a number of analyses, executed by different chemists, in order to determine not only the nature but the proportions of foreign matter, to the presence of which they owe their medicinal qualities.

In looking over these analyses we have found that experiments have been made upon different quantities. Some chemists have given us the solid and gazeous contents of a pint, some of a quart, and others of a gallon. In comparing them, therefore, with each other, the reader, in order to determine the relative proportions, will have to reduce them to a common measure, and, as this is somewhat of an irksome business, we have undertaken to relieve him from the task, and give the amount in *one hundred cubic inches* of water.

BALLSTON.

1. *Public Well.*

100 cubic inches of water yielded of

Muriate of soda	72·72 grains.
Muriate of magnesia	3·03
Muriate of lime	5·62
Carbonate of lime	16·01
Carbonate of magnesia	20·34
Oxide of iron	1·73
	118·75
Carbonic acid	105·62 cubic inches.
Azote	4·59
	110·21

Spec. grav. of the water 1008
Temperature 52° Fah.

MEADE.

Muriate of soda	68·83 grs.
Carbonate of soda	3·89
Carbonate of lime	32·68
Carbonate of magnesia	1·08
Carbonate of iron	3·03
	109·51

Carbonic acid - - -	90·90 cub. inch.

Temp. 50 Fah.

Steel.

Muriate of soda - -	65·30
Carbonate of lime - -	46·36
Muriate of magnesia - -	26·34
Muriate of lime - -	10·53
Carbonate of iron - -	8·44
	156·97
Carbonic acid - - - -	300 cub. inch.

Anonymous.*

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2. *Low's Well.*

Muriate of soda - -	74·46 grs.
Carbonate of lime - -	23.37
Carbonate of magnesia -	12·98
Muriate of lime - -	7·35
Muriate of magnesia - -	4·76
Oxide of iron - -	1·73
	124·65
Carbonic acid - -	104·76 cubic inches.
Azote - - -	4·59
	109·35

Spec. grav. 1008.
Temp. 52° Fah.

Meade.

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* The author of this analysis is not known; it was made at Paris, as it is said, by an eminent chemist, and the results were published in the New York Med. Repos. for 1808. The quantity of water employed amounted to 25 ounces, which, if of Troy weight, is equivalent to 47·450 English cubic inches, and he obtained 31 grains of common salt. Hence it is easy to find the proportion in the common measure we have adopted; for as 47·450 : 31 : : 100 : $x = 65·30$, and in the same way may be ascertained the relative proportions of the other substances. Of this analysis we shall say something hereafter.

Muriate of soda	61·47 grs.
Carbonate of soda	4·32
Carbonate of lime	27·92
Carbonate of magnesia	0·64
Carbonate of iron	2·59
	96·94
Carbonic acid	95·23 cub. inch.

Temperature 52° Fah.

STEEL.

SARATOGA.

1. *Congress Spring.*

Muriate of soda	178·35 grs.
Muriate of lime	5·62
Muriate of magnesia	8·22
Carbonate of lime	47·61
Carbonate of magnesia	29·43
Oxide of iron	0·86
	270·09
Carbonic acid	114·28 cub. inch.
Azote	3·46
	117·74

MEADE.

Muriate of soda	204·11 grs.
Carbonate of lime	77·26
Carbonate of soda	7·14
Carbonate of magnesia	1·45
Carbonate of iron	2·67
	292·63
Carbonic acid	148·48 cub. inch.

STEEL.

Muriate of soda	184·74 grs.
Carbonate of soda	6·06
Carbonate of lime	62·33
Carbonate of magnesia	12·64
Silex	2·59
	268·36

Dana.

Sulphate of soda.
Muriate of soda.
Carbonate of soda.
Carbonate of magnesia.
Carbonate of iron.
Silex.

Cutbush.

2. *Flat or Round Rock Spring.*

Muriate of soda	70·99 grs.
Carbonate of lime	26·83
Carbonate of magnesia	17·74
Muriate of magnesia	6·92
Oxide of iron	0·43
	122·91
Carbonic acid	114·28 cub. inch.
Azote	1·73
	116·01

Meade.

Muriate of soda	84·32 grs.
Carbonate of soda	4·84
Carbonate of lime	46·32
Carbonate of magnesia	0·64
Carbonate of iron	3·24
	139·36
Carbonic acid	109·31 cub. inch.

Steel.

Carbonate of soda - -	9·00 grs.
Muriate of soda - -	59·91
Carbonate of lime - -	65·80
Carbonate of iron - -	2·94
	137·65
Carbonic acid - -	69·29 cub. inch.

SEAMAN.*

The waters of other springs have also been analyzed, particularly by Drs. Meade and Steel, but the above, we presume, will furnish sufficient examples to the curious reader, upon which to institute a comparison, not only as regards the nature, but likewise the proportions of the substances which constitute the active ingredients of these mineral waters. Now, then, let us look a little into these analyses, and endeavour to make out the points in which these gentlemen coincide or disagree.

A single glance at the results of these analyses, is sufficient to shew that they differ exceedingly from each other, not only in relation to the salts, which are the products of evaporation, but to the relative proportions in which they exist. Of the muriates, Dr. Meade finds those of soda, lime, and magnesia, while Dr. Steel denies that the waters contain any other than common salt; on the contrary, the latter discovers carbonate of soda, which could not be detected by the former. The French chemist obtains two muriates and two carbonates. So much for the waters of Ballston. The analyses of those of Saratoga are equally discordant: Drs. Meade, Steel, Dana, and Seaman, find carbonate of Soda; Dr. Meade alone discovers the muriates of lime and magnesia, and Mr. Cutbush sulphate of soda. They all agree that the carbonates of lime, magnesia, and iron, and the muriate of soda, invariably constitute a part of their solid contents.

If we now turn our attention to the numbers which represent the proportions in which these saline bodies are present, we shall find them equally at variance; and, to state this fact more simply and clearly, the decimals shall be rejected, except when the quantity of a substance is very small. Let us take, then, as examples, the muriate of soda, the carbonates of lime, magnesia, and iron, and carbonic acid.

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* Dr. Seaman's analysis was made on the solid contents of ten pounds of the water, and we have estimated the above proportions upon the supposition that one pound (or 12 ounces Troy) is equivalent to 28·875 cubic inches.

BALLSTON.

1. *Public Well.*

	M.Soda.	Carb.Lime.	Carb.Mag.	Oxid.Iron.	Carb.Acid.
Meade	72	16	20	1·73	105
Steel	68	32	1·08	3·03	90
French	65	46		8·44	300

2. *Low's Well.*

	M.Soda.	Carb.Lime.	Carb.Mag.	Oxid.Iron.	Carb.Acid.
Meade	74	23	12	1·73	104
Steel	61	27	0·64	2·59	95

SARATOGA.

1. *Congress Well.*

	M.Soda.	Carb.Lime.	Carb.Mag.	Oxid.Iron.	Carb.Acid.
Meade	178	47	29	0·86	114
Steel	204	77	1·45	2·67	148
Dana	184	62	12		

2. *Round Rock.*

	M.Soda.	Carb.Lime.	Carb.Mag.	Oxid.Iron.	Carb.Acid.
Meade	71	26	17	0·43	114
Steel	84	46	0·64	3·24	109
Seaman	59	65		2·94	69

It is difficult to account for the great difference observed in the proportions above stated. Very considerable errors must exist somewhere; for the amount of the whole saline mass obtained by evaporation, does not vary in any great degree, and it is therefore improbable that the strength of the waters should, at different times, fluctuate to such an extent as to afford Dr. Steel 77 grains of carbonate of lime, and Dr.

Meade only 47 grains; or that the latter should find 29 of carbonate of magnesia, where but one grain and a half were obtained by the former. We can more readily believe that the two carbonates might have been confounded, or that the acicular crystals of sulphate of lime, formed by the analytical method which was pursued, might, perhaps, have been mistaken for sulphate of magnesia.

It is strange, also, considering the general uniformity in the modes of analysis, that one should detect the muriates of lime and magnesia, and find no trace of carbonate of soda, while three others state the carbonate as one of the ingredients, but are totally silent respecting the muriates. In order to reconcile these opposing authorities, it will be necessary to look into the analyses, and endeavour, if possible, to discover the source of these discordant results. First, then, of *carbonate of soda*. It was discovered by Dr. Dana, by pouring upon the dry solid contents of the Congress Spring water, two ounces of pure water; the solution, after filtration, was evaporated to dryness; upon the mass was poured distilled vinegar, which caused an effervescence; the solution was evaporated to dryness, mixed with alcohol of the specific gravity of 815, and, after standing a sufficient time, was filtered. The insoluble part was muriate of soda. The alcoholic solution was then evaporated to dryness, and the mass, by exposure to heat, was converted into a carbonate, which, when dissolved in water, and purified by nitrate of silver from a small portion of muriate, proved by subsequent examination to be carbonate of soda. Dr. Steel, on adding infusion of red cabbage to the solution which was formed by pouring water upon the mass obtained by evaporating a certain volume of the mineral water, states, "that it gave the whole a beautiful green colour; and, having previously ascertained the number of grains of carbonate of soda required to neutralize a given number of drops of dilute muriatic acid, he added this acid, drop by drop, to the water, until the original colour of the infusion was restored, and then, by subsequent evaporation, he obtained nothing but entire crystals of muriate of soda. This experiment, although rather coarse compared with the method usually practised to prove the presence and the proportion of carbonate of soda, was sufficient to shew its existence. Dr. Meade obviously detected the presence of an alkali, which must have been the carbonate of soda; for, in the analysis of the water of Low's well, he found that paper, stained with turmeric, had its colour changed to an orange brown; but, prepossessed by an opinion, which he derived from Bergman and Kirwan, that fixed alkalies and earthy salts are incompatible with each other, he concluded that the alkali which produced this change in turmeric, existed only in minute quantity, and neglected to ascertain either its nature or proportion; (p. 35). There is every reason to believe, however, that analysis does not always afford the ingredients which pre-existed in the water; that salts, which have been supposed incompatible as sulphate or carbonate of soda, with muriate of lime or magnesia, may exist as such, at a certain degree of dilution, and that, as the solution is concentrated, a mutual exchange of principles may take place, and new salts be formed. If, therefore, the muriates of lime and magnesia were actually dissolved in the water, they might re-act upon the carbonate of soda during the evaporation, and the resulting products would be muriate of soda, and carbonate of lime or magnesia, or both. This mode of viewing the subject might satisfy us respecting the reason why Dr. Meade failed in obtaining carbonate of soda; but then, directly in the face of this argument, we have the assertion of Drs. Seaman, Dana, and Steel, and of Mr. Cutbúsh, that this salt is actually obtainable, in notable quantities too, from the solid contents of the Saratoga waters, and there appears no reason to doubt of their correctness. How to reconcile these opposite results we know not, and shall therefore leave the mystery to be cleared up by those who are more immediately interested in the business. In the second place Dr. Meade invariably found, in his alcoholic solutions, muriates of lime and of magnesia. Dr. Seaman and Dr. Dana saw no indications of these salts, and as to Dr. Steel, he roundly asserts that alcohol, purified by muriate of lime, could not be made, by him, to take up the smallest quantity of the saline property from the residuum of any of the waters, p. 93. This discordance is very extraordinary. One would suppose, that, in an experiment of this kind, which consists only in adding alcohol to a saline mixture, filtrating and subsequently evaporating, no difference in opinion could possibly exist, whether a residuum was or was not obtained. Yet here one chemist not only finds that two salts are dissolved, but, by certain processes, fixes with precision their relative proportions; while another is equally clear, that all this must be an illusion, and that no salts soluble in alcohol can be procured from the solid contents of these waters. The analysis of the French chemist favours the conclusions of Dr. Meade; but we know not the authority upon which, in this instance, we may depend, and the gross and palpable error which has been made in the estimate of the volume of carbonic acid, and the weight of the carbonate of iron, would lead us to doubt the correctness of the whole examination. The weight of evidence against the existence of the muriates in the residue of these waters is, when numbers are considered, as four to one; but as we cannot at present reconcile these opposing authorities, we shall take the old adage as an apology, and be silent. It is possible that carbonate of soda, and the muriates, even perhaps that of iron, may exist together in the waters of the wells, and that, during their concentration, they mutually decompose each other, the salt of soda being present in larger proportion than is necessary to produce this effect, and the excess consequently constituting a part of the residuum. As for the *sulphate* of soda, discovered by Mr. Cutbush, we think there must have been some mistake; for all those who have examined the waters, although they disagree in almost every thing else, are unanimous in the opinion that no indications of sulphuric acid are to be perceived. We shall now say something of the quantity of oxide or carbonate of iron, and of carbonic acid, procured by these gentlemen. On looking at the tables, it will be seen, that, from the waters of the public well, Ballston, Dr. Meade obtained 1. 73 gr.; from Low's the same quantity; from the Congress spring, Saratoga 0·86 of a grain, and, from the water of Flat or Round Rock, 0·43 of a grain. From the same waters and the same volumes, Dr. Steel succeeded in separating 3·03; 2·59; 2·67; 3·24 grains of oxide of iron; whence it will appear, that where Dr. Meade found the smallest, Dr. Steel discovered the largest quantity; and, conversely, that, where the latter separated the smallest, the former procured the largest quantity. Dr. Meade's method of obtaining the iron was, to expose the solid contents of the water (e. g. of Low's well) to the action of air and of moisture for three weeks, until the iron was "*reduced*" to the highest state of oxidation, and then to separate the other ingredients by the action of acetic acid. The metal is thus converted into the peroxide, and the author quotes Mr. Chenevix as saying, that it is then in the fourth or highest state of oxidizement, and weighs as 189 to 100 of metallic iron. We were not a little surprised at finding that Dr. Meade took the oxide, thus formed, as the same with that which existed in the water, and that he should have copied such a mistake as the one which he gives on the authority of Mr. Chenevix. He exposed the protoxide of iron, for three weeks, to the air, in order that, by converting it into the peroxide, it might be rendered insoluble in acetic acid. During this change, it absorbed one proportion of oxygen, and the peroxide, thus formed, was weighed and recorded in his table, as the quantity found in the water. Dr. Meade ought to have known that there are but two oxides of iron,* the black and the red, the former being considered as the protoxide, and the latter as the peroxide; and that, in each case, the metal being represented by 100, the oxygen, in the first, must be considered as 30, and in the last as 45; and not 89, as he has stated. The real quantity of oxide of iron, dissolved in the water, would amount to about 0·90 of a grain. The same correction must be made in the weight of oxide stated in the other analyses. Dr. Steel appears to have estimated the quantity of iron by the weight of the precipitate produced by prussiate of lime; but the details of the process are not given. From the great difficulty of forming and of preserving the simple prussiate, we are inclined to suppose that the triple prussiate was employed, in which case, unless an allowance were made for the iron existing in it, the amount of this metal in the water must have been overrated. This perhaps may account for the difference in the weight of this ingredient observed in his and Dr. Meade's statements.

With respect to the volume of carbonic acid contained in the waters, a very considerable difference will also be found to exist between the numbers of Drs. Meade and Steel. The former, in his examination of four of these liquids, states them as follows: 105, 104, 114, 114 cubic inches; while the corresponding analyses of the latter give 90, 95, 148, 109, in 100 cubic inches of the water. The method practised by Dr. Meade to separate this volatile product, was to fill a vessel of a certain capacity with the water, and to invert over its orifice a decanter graduated into cubic inches, and filled with water at the temperature of 120°; the vessel was then placed in a sand bath, the contents were made to boil, and the ebullition was continued until the gas ceased to be extricated. After allowing the water and the air in the receiver to cool to 60°, the number of cubic inches of water, which had been displaced, was noted, and the purity of the gas was tried by lime water. Dr. Steel caught the gas in a bladder, and then passed it into a graduated vessel, filled with water, at the temperature of 70°. In three analyses out of four, he obtained less of carbonic acid than Dr. Meade; and here the difference, which is not very considerable, may be accounted for by the probable absorption of a portion of gas, both by the bladder (for we presume it was moist) and by the water, through which it was passed. But from, the water of the Congress spring, Dr. Steel obtained no less than thirty-four cubic inches more of carbonic acid than Dr. Meade. Our mode, therefore, of explaining this discordancy must here fail, and we are under the necessity of again referring for some explanation, on this head, to the gentlemen who are most interested in the result.

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* M. Guy Lussac asserts that he has discovered a third oxide, containing oxygen in proportion intermediate between those of the black and red; but the fact has been disputed.

Dr. Steel affirms, that the elastic fluid, procured by his process, was pure carbonic acid, and that no other gaseous product could be detected. By Dr. Meade, on the contrary, it is stated, that a notable proportion of azote was found mixed with the carbonic acid; and he has accordingly, in his analysis, uniformly recorded this gas as one of the aerial ingredients of these mineral waters. Although azote has been found in similar waters in Europe, and we think it not very improbable that it may be present in those of Ballston and Saratoga, the mode adopted by Dr. Meade to obtain it, was not so satisfactory as to convince us of the truth of his assertion. The gas was caught over water heated to 120° ; after cooling, it was repeatedly passed through fresh lime water, and, after being in contact with the liquid until the absorption ceased, there remained a small proportion of gaseous fluid, which was inferred to be azote, because it extinguished flame. Now we believe that the atmospheric air, contained in water, is not all evolved by heating it to 120°; to effect this separation it requires boiling. Supposing, then, that a portion of common air remained, it would be displaced by the carbonic acid, and would reassume its elastic form, and, when mixed with one-seventh of its volume of carbonic acid, it would be rendered incapable of supporting combustion. This, however, we offer merely as a conjecture. It would have been easy to have ascertained the presence of atmospheric air, by the mutual action of nitrous gas, and the oxygen which it contains; an experiment which is performed over water, and, consequently, does not require the aid of a mercurial apparatus, with which it appears Dr. Meade was not supplied.

But it is time to turn our attention to a subject which will probably be considered by a majority of our readers of much more consequence than that with which we have hitherto been occupied; and that is, the medicinal qualities of these waters. We shall, therefore, close this view of the analysis with the remark, that the discordance exhibited throws a shade of obscurity over the real ingredients of these waters, and that it would puzzle the physician, who sends his patients to the springs to decide, whether *he* prescribes muriates or carbonates, or *they* are to drink any thing more than a solution of common salt, chalk, magnesia, a little iron, and a great deal of carbonic acid.

It seems, however, to be agreed by physicians in Europe, who have attended to the medicinal effects of mineral waters, that their operation is more salutary than that of any artificial combination, possessed of the same chemical properties. We shall not, therefore, deny to the mineral waters of Ballston and Saratoga the credit of many wonderful cures of various diseases, marked by totally different and even opposite characters; nor do we much care to ascertain, whether these cures are wrought by change of air, exercise, temperance, the influence of expectation, or by some mysterious virtue in these natural combinations. The following are Dr. Steel's remarks on the effects of the waters in the diseases, for which they have been most celebrated:

" The waters are so universally used, and their effects so seldom injurious, particularly to persons in health, that almost every one who has drank of them assumes the right to direct their use to others, and even empiricks, without any knowledge of their composition, and little or none of their effects, contrive to dispose of their *directions* to valetudinarians, to no other purpose than to injure the reputation of the waters, and destroy the prospects of the diseased.

" Nothing can be more absurd than the idea that governs many who visit the *springs* for the restoration of their health, that they are to recover in proportion to the *quantity* they drink; for although persons in health may, and frequently do, swallow down enormous quantities of the water with impunity, it by no means follows, that those whose stomachs are enfeebled by disease, can take the same quantity with the same effect. Stomachs of this description, most frequently, reject the too copious draught, and save the system from the evil consequences that would otherwise inevitably follow; but when it happens to be retained, the result is indeed distressing; the pulse becomes quick and feeble, the extremities cold, the bowels swollen and painful, and the whole train of nervous affections alarmingly increased; and, should the unfortunate sufferer survive the effects of his imprudence, it is only to a renewal of his worst apprehensions from a loss of confidence in what he most probably considered a last resort.

" Among the great variety of invalids who resort to the springs, none, perhaps, receive more essential and effectual benefit from their use, than *the Bilious and Dyspeptic.* In the first, if the attack be recent, and unattended with any serious organic affection, it is most usually removed, in the course of a few days, by a free use of the Congress water; but in those cases where the functions of the stomach and bowels have become impaired, from the long continuance of the disease, attended with anasarcous swellings of the extremities, &c.; although the waters of this fountain may be resorted to with nearly the same assurance of obtaining relief, nevertheless, more caution is indispensably necessary in its administration; for, should a great quantity of the water be drank, without having the proper effect by the bowels and kidneys, it is never beneficial; but, on the contrary, frequently increases the most alarming symptoms of the disease. In this case I have been in the habit of recommending the conjunction of some mild cathartic medicine; and, for this purpose, two or three grains of *calomel* have been given over night, followed in the morning with three or four tumblers of the water, with the happiest effect; a few doses of this description, usually places the bowels in a situation to be more easily wrought upon by the water alone, and the patient becomes convinced of its efficacy in his disease, from a few days proper application.

" In Dyspepsia, it is usual to begin a course of the waters with the Congress. This should be taken in the morning before breakfast, four or five tumblers full are commonly sufficient to produce a pretty copious discharge from the bowels, and, in weak irritable habits, half the quantity, or a single tumbler full, in some cases, is amply sufficient to answer the purpose; but in those cases where the bowels are attended with an habitual constipation, the quantity of water required to move them is apt to prove too cold to the stomach, and, by producing cold chills and nausea, frequently defeats the general intention of its application; this may be prevented by taking some suitable laxative over night, and a much less quantity of water in the morning will answer the wishes of the patient, without subjecting him to any inconvenience. But the water of the Congress is not, alone, to be depended on for the removal of this disease; when the stomach and bowels have been sufficiently cleansed by the pleasant and innocent purgative properties of this water, recourse must be had to the operation of the more powerful *chalybeates;* these are to be found in the waters of the *Flat Rock*, the *Columbian*, in *Ellis's Spring*, and at the *Spa.*

" The quantity of waters, from either of these fountains, to be used daily, depends, in a great measure, on the state of the disease and the disposition of the stomach; it is therefore necessary to commence their use in small quantities at a time, in distant and regular intervals, gradually increasing the quantity and frequency of the draught, as may be most agreeable to the stomach, and least injurious to the feelings. In this way, the quantity may be increased to from one to two quarts a day, and it is questionable whether a much larger quantity may be drank with any additional advantage.

" Conjoined with the internal use of the water, bathing should not be forgotten; its exhilarating effect upon the surface, contributes much to the restoration of the vigour and health of the stomach. The cold shower bath should always be preferred where the energy of the system is sufficient to overcome the effects of the cold, and produce the sensation of warmth over the surface of the body, immediately after its application. Where this sensation is not produced, the cold bath should be dispensed with, and the tepid, or warm bath, substituted in its stead, together with general friction, with a flesh brush or coarse flannel, over the whole surface.

" The stimulating effects of these waters, arising from their saline and gaseous properties, give them a decided preference over any other, as a bath; and those who are labouring under a deficient or irregular action of the cutaneous vessels, arising either from a sympathetic affection with a diseased stomach, or from an original affection of the vessels themselves, will find it to their advantage to persevere in its use under this form.

" The idea of bathing before sunrise, or early in the morning, is entirely erroneous. Before bathing, the system should always receive the invigorating effect of moderate exercise and a nutricious repast. The hour of ten or eleven in the forenoon is, therefore, the most suitable time for its application.

" In calculous and nephritic complaints the waters have long been celebrated for their efficacy; and numerous well attested instances of their good effects can be produced, where the disease was not only mitigated, but effectually cured. In these cases the subjects of them voided large quantities of sand and small gravel; and, for some years past, have felt no symptoms of the return of the complaint.

" The fountain that would seem to promise most in these diseases, are the Hamilton, and Taylor's Washington, as they contain the greatest quantity of the *aerated alkali*. But the waters have been usually drank indiscriminately for this purpose, without reference to any particular fountain; it is, therefore, probable, that the fixed air and lime add to the *lithontriptic* properties of these waters.

" They should be drank in such quantities as to keep the bowels loose, and repeated sufficiently often to keep up an increased secretion by the kidneys.

" In chronic rheumatism, the virtues of the waters were celebrated by the aborigines, and later observations confirm the justice of their faith. The Congress water has the most celebrity in this disease. It should be drank in the morning, in sufficient quantities to move the bowels two or three times, followed, through the day, by moderate draughts of some of the other fountains; and, in most instances, the *shower bath* will add much to the efficacy of the water. Following this course, for a length of time, gradually relaxes the rigidity of the muscles, adds strength, and facility of motion, to the diseased joints, and restores ease and vigour to the whole system.

" *Scrofula* is another disease for which those who are afflicted with it frequently become applicants to the waters, and experience has sanctioned the belief of their utility in this afflictive complaint. The chalybeate waters are those from which we are to look for the greatest benefit; they must be commenced in small doses, and the quantity gradually increased as the stomach will bear them, and their use continued at least through the summer months. There are but few of this description that have not received advantage; and numerous instances might be adduced, where the less seriously affected have perfectly recovered, in consequence of a proper course of bathing and drinking.

" In dropsy, arising from viceral obstructions of long continuance, the waters are manifestly injurious, as they invariably increase the swelling and add to the sufferings of the patient; but, in recent cases, where the affection arises simply from a deficient action in the absorbent vessels, the water has a singular effect in removing it; it should be drank in the morning freely, so as to produce a copious discharge from the bowels, and, through the day, taken in such quantities as to keep up a pretty constant discharge of urine. The bloating is relieved immediately, and a subsequent course of chalybeates, will finally establish the permanency of the cure.

" In *Paralysis* the waters have usually been singularly serviceable; the purgative properties of the Congress render it most applicable to this disease, and its good effects are much increased by the use of the bath.

" In chlorosis, and a variety of other complaints peculiar to the female sex, the waters maintain a *high* and *deserved* reputation. In these cases the bowels should be kept loose by the use of the more purgative waters, and the stronger chalybeates should be persevered in for a length of time; their good effects will be accelerated by frequent bathing and moderate exercise."

Dr. Meade not only presents us with a very copious account of the medicinal properties of the spring waters, but also occasionally steps out of his way to give his views of diseases and modes of cure. In treating of dyspepsia, he has afforded us some novel and very entertaining remarks on a complaint which has of late years appeared in Boston, which he has thus described:

" In Boston, in particular, of late years, a disorder has prevailed, to which the faculty have given the name of dyspepsia; to doubt the propriety of which, would seem to be an instance of some presumption. As far as my observations have gone, a complaint somewhat similar is there a very frequent disease. It principally makes its attacks at a very early period of life, reducing the patient to the utmost state of emaciation and debility: many of the symptoms are such as are usually observed in dyspepsia; they are continued for a length of time, and are aggravated in the winter months. Children of the age of ten or twelve years are not exempted from it, and some of the finest young men in the country are attacked with it from the age of fifteen to twenty-one. It soon reduces the patient to a state of the utmost debility and emaciation, such as is usual in the last stage of phthisis pulmonalis; the countenance is altered, the cheeks are prominent, the eyes look hollow and languid, the hair often falls off, the nails are of a livid colour, and the pulse becomes so low, that I have, in one instance, been able to count only forty-five strokes in a minute. The persons most liable to this complaint are remarkable for a particular formation; a long neck, prominent shoulders, narrow chest, clear skin, and thick upper lip, with other marks of a scrophulous diathesis. They have no cough, nor can any suspicion be entertained of an affection of the lungs during the whole of the disease. If I am correct in those appearances, have we not much ground for suspicion that there is a scrophulous disposition in the system, and that the whole of those symptoms arise from an obstruction in some of the lymphatic glands in the neighbourhood of the stomach, or in those of the mesentery, similar to incipient tubercles of the lungs, and rendering them incapable of conveying chyle or nourishment to the system? thus arises that emaciation which constitutes a species of marasmus, and is so similar to that which occurs in the latter stage of consumption."

Now we have no doubt that Dr. Meade may actually have seen such a train of symptoms as he describes, at least in two or three individuals. The *extreme debility* and *emaciation*, the *altered countenance*, the *prominent cheeks* (*cheek bones*), the *hollow* and *languid eyes*, the *falling hair*, and the *livid nails*, have actually appeared, and Dr. Meade may have witnessed them, in at least *two* and possibly *three* instances. But after casting our eyes about us, to ascertain whether a disorder, with these symptoms, to which the faculty have given the name of dyspepsia, has really *prevailed* in Boston of late years, we must confess that none such has been discovered. The disease which the Boston physicians have ventured to call dyspepsia, does not make its attacks *at a very early period of life*, but generally in subjects between twenty-five and forty-five years of age. It is not *aggravated in the winter months*, but is usually mitigated in these months, and troublesome in hot weather. This complaint is often relieved by travelling, and by change of climate, but not by change to a *warm climate*. The persons who have had this disorder *have not been remarkable for any particular formation;* nor can we trace a common resemblance between them in any one point. This dyspepsia has appeared in young men of five and twenty, who have been careless of their regimen; in robust gentlemen of forty-five, who have habitually lived too well; in married ladies, whose constitutions have been impaired by inattention to exercise, and too great an exertion to nourish their infants. People with thick upper lips, narrow chests, and clear skins, do not appear to have been more troubled with the dyspepsia than those with thin lips, dark skins, and black hair. Hence it would seem probable that the disease in question is not of a scrophulous nature; a probability which is strengthened by the facts, that, in the moist climates of Great Britain and Holland, where scrophula is the most common of diseases, we do not find enrolled in the long list of dyspepsiæ *symptomaticæ* and *idiopathicæ* of Cullen and Sauvages, any disease under the name of Dyspepsia Scrophulosa; while, in the dry air of Boston, where this new kind of dyspepsia is said to exist, it appears that scrophulous disorders are comparatively rare. We, therefore, venture to conclude, with deference to the authority and experience of the author,

1. That this disease is not of a scrophulous nature, since it does not occur in scrophulous climates nor constitutions.

2. That, since it arises from the same causes as dyspepsia; exists in the same kind of subjects; exhibits the same symp-

toms, and is cured by the same means—it is neither more nor less than a genuine dyspepsia.

If it be asked, whether indigestion be actually more prevalent in Boston than other American towns, where good living is equally common, we should make our reply with due circumspection. We should allow, that a few severe and remarkable cases have existed, and excited much attention both here and at the Ballston springs; but might add, that we have noticed ladies and gentlemen, of other cities than Boston, who were travelling about in search of health, under the severe affliction of *bilious* complaints, which, to our eyes, exhibited a considerable resemblance to the Boston dyspepsia. It would look like presumption to say, that, in such cases, the mischief is more likely to proceed from a weak stomach, than a corrupt liver; but, at least, it will be generally admitted by impartial people, that of all our organs, there is none more commonly ill used and over-strained than the gastric viscus. Possibly we might be disposed to go a little farther, and allow, that the stomach, in the inhabitants of Boston, is endowed with a more obstinate and reluctant character than that of other people; whence, instead of exhibiting its revenge for abuse, in a slow and gentle manner, on our ligaments and membranes in the form of gout, or on the liver in the shape of *bilious* complaints, it enters at once into a contest with the articles of food that are presented to it, and either repels them without ceremony, or converts them into biting acids, and horrible explosive gases, which produce the formidable train of symptoms described by Cullen—" Anorexia, nausea, inflatio, ructus, ruminatio, cardialgia, gastrodynia," &c. and sometimes even the more formidable phenomena, so eloquently represented by Dr. Meade.

These treatises are meritorious for the industry and science they exhibit, and the judicious advice to the public, respecting the use of the mineral waters; but it is to be regretted, that they are all deficient in a geological map of the country in which the springs are situated.

A Treatise on Verminous Diseases, preceded by the natural history of Intestinal Worms, and their origin in the Human body. By VALERIAN LEWIS BRERA, *Professor of Clinical Medicine in the University of Pavia, ornamented with five plates. Translated from the Italian, with notes, by Messrs.* J. BARTOLI, M. D. &c. *and* CALVET. *Paris,* 1814.

Translated from the French, with additions, by John G. Coffin, M. D. 1816. *One volume 8vo. Boston, Cummings and Hilliard,* 1817.

THIS is the production of an Italian physician of much eminence; has been translated from the Italian into French, and has now received an English dress, from a gentleman to whom we have been before indebted for similar labours. The translation is very faithful, and the work is considerably enlarged and enriched with additions from various sources, by the translator. It is accompanied with plates, which are accurate and handsome copies of those attached to the original work, and do honour to the American artist.

The work is divided into four lectures, with copious notes, containing numerous references to writers on the several divisions of the subject.

Lecture First.

The first lecture treats of the natural history of that class of worms, which inhabit the human body. This class consists of the following genera, viz.:

1st. Tænia.
2d. Vermis Vesicularis.
3d. Triocephalus.
4th. Ascaris Vermicularis.
5th. Lumbricoïdes.

A description of each genus is given, of which we will attempt a short abstract, as nearly as possible in the words of the translation.

1st. The tænia. This is a very long worm, which seems to be formed by a chain of flat articulations, united together by means of a border or edge, varying in breadth and thickness. It is divided into head, neck, body and tail. The head is sometimes so small, that it cannot be distinguished without the microscope. It resembles a small tubercle, and rises on the anteriour extremity of its body, called neck. It is furnished with four apertures, which are eminences in some worms, and depressions in others. From each of these four openings, proceeds a canal for nourishment, which extends to all the articulations. In the human tænia, a central tube has been observed, which the animal has the power to elongate, and which also is an instrument for nourishment, and is supposed to be the mouth of a canal, which extends to all the articulations. Some naturalists suppose that nourishment is likewise obtained by means of lateral papillæ, with small mouths, situated on the edges of each articular internode. The neck is formed by the assemblage of small articulations, which become broader, longer and larger, as they approach the body of the worm. Those of the body increase, till they arrive at the middle point between the two extremities, when they diminish, but not in the same proportion. But it may be said, that the joints of the tænia diminish in size, as they recede from the centre to either extremity of the animal. The neck, however, is always the smallest part. The tail terminates by a piece quite truncated, or cut off and raised on its sides in the form of two hooks, rounded at their summit.

The tænia is oviparous, and is supposed to be hermaphrodite. It is very tenacious of life, and sustains a high degree of temperature, before it is destroyed. It usually occupies the small intestines, but is occasionally found in the stomach.

This genus is divided into two species, the armed and the unarmed tænia. The first species, or armed tænia, is also called tænia cucurbitina, or solitary worm. It varies in its appearance, according to its age, food, &c. but its specific character is designated by the presence of two pointed and protuberant appendices, with which the head is armed, and which, by analogy, are called crotchets or fangs ; these can be seen, by the naked eye: By the aid of the microscope, and by stretching the head a little, these fangs are seen to be extended into a small crown, perfectly circular, and stellated ; and in the centre of this is the central tube mentioned above. By means of these fangs the worm fixes himself with great pertinacity to the mucous root of the intestines, and produces in the part sharp pricking, one of the distinguishing marks of the presence of this species.

The second species, or unarmed tænia, commonly known by the name of tænia lata, differs from the other, principally by the absence of the crown of fangs attached to the head of that species. But it is also flatter, somewhat resembling a ribbon ; it is whiter, and its articulations are less distinctly marked. Its head is very minute, and, in general, the joints of the neck are very thin, being almost imperceptible.

The second genus is what our author denominates the vesicular worm. The only instance, in which he relates to have seen this animal, was in the brain of a man, who died in consequence of some uncommon affection of the head. In the lateral ventricles of this brain, were discovered "two large clusters of hydatids, extending along the branches of the plexus choroides, to which they were so intimately attached," that they could not be separated without tearing the substance of the plexus. "This collection of hydatids being removed from the brain, and attentively examined, we saw," says the writer, "that each little bladder contained a real worm, of a structure quite singular. It was composed of a head similar to that of the tænia, and a vesicle full of water." In fact it was supposed, that each little bladder was the body of a worm, and that the addition of a head, situated without the bladder, completed the hydatid. It is thought "that the greater part of hydatids may be reduced to real vesicular worms." The American translator has added to the fourth lecture, an interesting history of hydatids from Monro's morbid anatomy. It is a valuable addition to this book, especially to those who have not access to the work of Monro; for whether we admit them to be animals or not, they constitute a disease of the human body; and it is, therefore, important to the physician to be familiar with their appearance and morbid effects. The following remark of our author, viz. "that it seems probable, from observations already made, that this worm commonly resides in those parts of the body, which abound in lymphatic vessels," and this note from Soemmerring, that "sometimes hydatids are true varices of lymphatic vessels," would seem to point to a much more probable source of their origin and peculiar aspect, than the supposition, that they are living animals.

The third genus is the triocephalus, or capillary head. It is an uncommon worm, and has been found only in the large intestines of some half-starved or sickly subjects after death. "Its form is that of a spiral line, and its largest part does not surpass half a line in diameter. Its length is from an inch and a quarter to two inches. The extremity terminating in a very fine filamentous elongation is ascertained to be the head of the animal. It is not hermaphrodite.

The fourth genus, the ascaris vermicularis, is more familiar to us; it is a round filiform worm, fine and slender at both ends, and from four or five lines to an inch in length. It has a surprising contractility, and to this we are probably to attribute those irritations of the intestines, and particularly the anus, which torment the sick, especially children. The surface of this worm is full of wrinkles, which seem to be formed by a multitude of rings. Its anterior extremity, or head, is obtuse; its posterior end, or tail, is slender and shiny. It resides principally in the large intestines, but it is occasionally found in other parts of the body, where the mucous fluids abound, as the bladder, stomach, œsophagus, &c. The sex is distinct in each individual.

The fifth genus, or lumbricoïdes, is very common. It is perfectly round, commonly about the size of a writing quill; most frequently from six to ten fingers breadth in length. Its colour is white, sometimes resembling flesh colour. The whole surface of the body is wrinkled and annular, and tapers to both extremities. The head is furnished with three hemispherical and pyramidal eminences, in the centre of which there is a triangular aperture, called its mouth. With these three protuberances, it fixes itself to the intestines, and by closing these in such a manner as to form a hard and sharp point, sometimes perforates them, and makes its way into the abdomen, gall bladder, urinary bladder, kidneys, &c.

Linnæus supposed this worm to be the same species as the lumbricus terrestris; but they are ascertained to be different in several respects, and particularly in their sexual arrangements; the first being male and female in different individuals, the last hermaphrodite.

Lecture Second.

The second lecture discusses the question of the origin of human worms; and the theory of equivocal generation is ingeniously opposed. The principal arguments are, 1st. That human worms are endowed with organs of procreation, in the same manner as other more perfect animals. This argument may be strengthened by the reflection, that the perfect order of nature does not permit any thing superfluous or unnecessary; and, therefore, if so efficient a means as spontaneous generation had been provided for the continuance of the species of these animals, they would not have been furnished with sexual organs.

2d. This theory is directly opposed to the general laws of nature, in the reproduction of other animals. There is no well ascertained instance of a resort to this mode of generation; and it is not reasonable to suppose, that a particular exception would be made in the production of this class of worms.

They are oviparous, and the same species are found in other animals, and, in fact, in those animals which make our common food. Hence the difficulty of their first introduction into the human body is very much lessened. The eggs being once swallowed, and favourably situated, the animal would soon be developed. But worms are found in the liver and kidneys of animals, and, they are said to have been found also in infants, who have died at birth. These accounts are not so well authenticated, as to be received without reserve; but, admitting them to be true, the difficulty, which they would seem to oppose, is obviated by the fact, that these eggs are so extremely small, that they may pass from the stomach and intestines into the circulation, and may thence be deposited in the most secret parts of the body. But we must refer readers to the work itself, for the details of this investigation. If there be difficulties on the side of the question advocated by our author, they cannot equal those, which attend the supposition of equivocal generation. No difficulty can be greater, than to conceive, that mere matter should so arrange its particles as to form an organized and living being, and even so as to maintain uniformly those definite and nice distinctions, which constitute the difference of species.

Lecture Third.

In this lecture we have an interesting account of the multitudinous symptoms produced by worms, of which it is impossible to present a just idea by any abstract, proper to be given in this place. Some of the common and most general symptoms, recorded in this lecture, are, a variable countenance, suddenly changing from red to pale, or leaden colour; swelling of the lower eyelids; eyes dull and sad, and the pupils evidently dilated; insupportable itchings in the nostrils; headache and sometimes delirium and phrenitis; uneasy sleep; grinding of the teeth; sometimes fainting and vertigo; dry and convulsive cough; difficult respiration; hard and rapid pulse; irregular appetite; emaciated body; and tumid belly, which is often the seat of severe pain.

Two most instructive cases will be found attached to this lecture, well worth reprinting, were not the book accessible to every one.

The particular symptoms, denoting the presence of each kind of worm, are next detailed. Those indicating the presence of tænia are, a sense of pain in the belly, produced by something alive; occasional pricking or biting in the region of the stomach; a sort of undulating swelling and subsidence of the belly; great appetite, but a gradually increasing emaciation and weakness, &c.

The presence of the ascaris vermicularis is indicated by a dull feeling of irritation, or a tedious and intolerable itching in the lower part of the rectum, where they principally reside. They often produce severe local, besides many of the general symptoms excited by the other kinds of worms.

The local symptoms of the lumbricoïdes are, a colic and a rumbling in the belly, which is often very tumid and hard; a pungent and rending pain, particularly in the umbilical region, caused by the attempts of the worm to perforate the intestines, &c.

The symptoms produced by the vesicular worm, and the triocephalus, are not so well ascertained. We must refer to the work itself for all that is known of them, and for many other particulars relating to the symptoms produced by other worms. We will, however, make one more extract from that part of this lecture, which treats on the sympathetic affections from worms: "Among other affections of this kind, the following formidable list should be noticed; viz. intermission of the pulse, palpitation of the heart, syncope, vertigo, loss of speech, blindness, buzzing in the ears, dejection of mind, stupidity, delirium, contractions in sleep, disturbed thoughts, inquietude, anxiety, hiccough, convulsions, epilepsy, apoplexy," &c. &c.

Lecture Fourth.

The fourth and last lecture is on the treatment of verminous diseases. Some general principles are first stated, which are to be observed in the treatment of these diseases. An examination is then made of the principal remedies from the vegetable and mineral kingdoms, which have either been extolled, or have been really useful for the expulsion of worms. Some of these, which have merit, are not used, and perhaps are little known in this country.

The treatment of tænia has received particular attention. Several celebrated methods for the expulsion of this worm are detailed; they have all, it seems, been successful under favourable circumstances; and though they will probably be often superseded by the use of the oil of turpentine, they should be treasured in remembrance, as resources in time of failure of this remedy, which may happen in cases attended with some uncontrolable peculiarities.

In this place the American translator has added a history of the introduction, and of the progress of the use of the oil of turpentine, as a remedy against tænia; the account is taken principally from the second volume of the Medio-Chirurgical Transactions. The evidence is strong in its favour, and it has continued to accumulate by the successful issue of new cases. It has been given to children in doses of from one to two ounces, and to adults from three to four ounces, and repeated at short intervals. It is, however, a remedy which should be employed with some degree of caution; for such doses as these may be considered extravagant.

For the removal of the ascaris vermicularis, enemas are the appropriate applications. For this purpose the following substances are recommended, viz. Muriate of soda, oleum ricini; all bitter substances, particularly the semina santonici. The American Translator mentions the oil of turpentine. Cathartics should be occasionally administered during the use of the enemas.

Among the many articles recommended for the expulsion of lumbricoïdes, the dolicos pruriens, or cowitch, is praised by the American Translator. This substance is supposed to act mechanically upon the worms, and to render their situation very uncomfortable, and is, at the same time, harmless to the coats of the stomach and intestines. It should be preceded by a cathartic, which should be occasionally repeated during the administration of the cowitch. For the same purpose as the cowitch, an amalgam of tin and mercury is commended. This form of amalgam was first introduced to notice by Dr. Fisher, President of the Mass. Medical Society, in the Medical Communications of that Society, vol. 1st.

Chaberts oil, it seems, is also an article of reputation for the expulsion of worms. It is the result of a distillation of one part of empyreumatic animal oil, and three parts of spirit of turpentine, mixed and allowed to stand four days. It is said, " No species of worms can elude the destructive force of this distilled oil." It is thought particularly useful against worms of animals; for instance, the bots of horses, &c.

Many miscellaneous facts concerning worms, which infest other parts of the body than the intestinal canal, are added by the American Translator; some of the most curious only can be referred to.—1st. An account of a method of curing the malis draconis, or guinea worm. 2d. History of the effects of worms in the bladder. 3d. Case in which the larvæ of insects were discharged from the eyes, nose, mouth, and ears. The eggs were first deposited in an issue on the back of the neck, and the maggots must have made their way along the cellular membrane; they caused the most distressing pains of the head. 4th. Case of a seven years almost uninterrupted headache, relieved at last by discharging from the nose a number of worms; they were induced to quit their residence by the application of assafœtida, taken as a snuff.

We cannot pretend to have noticed all the valuable contents of this volume; nor was it our intention to do so. It is a book which every physician ought to purchase, not only for its own worth, but to reward the gentleman, who has laboured at its translation, and to encourage the publication of valuable medical works on our side of the waters.

Medico-Chirurgical Transactions, vol. vii. part ii. 8vo. pp. 300. London, 1816. Longman and Co.

[From the London Medical Repository.]

IN our analysis of the first part of this volume, we noticed the papers it contained, not in the order of their arrangement in the work, but according to the affinity of their subjects; and we purpose to adopt the same plan in our examination of the part now before us. It contains eighteen essays.

The only anatomical paper is the *Description of an Extra-Uterine Fœtus contained in the Fallopian Tube;* by George Langstaff, Esq. surgeon. The patient "was forty years of age, had borne five children, and was delivered of a full grown living child seven months previous to the last stated conception." Her dissolution was sudden, without being preceded by any illness, and the only symptoms which marked the fatal attack, were "a violent pain in the lower part of the abdomen, accompanied with sickness of stomach, and a disposition to faint." On opening the abdomen, two quarts of blood were found, "chiefly in a fluid state, covering the intestines, and occupying the pelvic region; and which, on investigation, were found to have come from the right fallopian tube, which "was dilated to about the magnitude of a moderate sized hen's egg, and had burst in two places." The enlargement was discovered to have proceeded from the increasing size of an ovum, which contained "a fœtus of about eight weeks, floating in the liquor amnii." On laying open the uterus, which was larger than in the unimpregnated state, the vessels were found to be large, but empty; in the cavity and neck was much gelatinous matter; and when this was washed off, the internal surface of the viscus looked very vascular;" but there was no decidua, and the cervix was not closed as it is during utero-gestation. In every other respect the body was free from any appearance of organic disease.

This case differs from others of a nearly similar nature on record, chiefly in having the cervix uteri open, "a decidua not having been formed." With regard to the cause of the laceration of the fallopian tube, and the obliterating of its canal, Mr. Langstaff thus expresses himself:—

"In consequence of the regular development of the ovum, the containing parts are distended beyond their capability, and become attenuated; progressive absorption takes place; the vessels supplying the ovum, which are necessarily enlarged, suffer in the destructive action; and from their not having been obliterated by adhesive inflammation, their coats are gradually absorbed, they burst, and the blood now finding free access to the sac, which is already rendered extremely thin, it bursts as in aneurism, the blood is propelled into the cavity of the abdomen, and thus life is extinguished.

"One of the most remarkable, and I believe unique circumstances in this case is, that no communication existed between the uterus and the ovum, the canal of the tube being completely obliterated; and it is also worthy of remark, that there was only the corpus luteum in the right ovarium, which has been described; all the ova, except this, having appeared to be produced by the opposite ovary.

"It would be impossible to state accurately, whether this obstruction took place, before the fallopian impregnation, or in consequence of it; but in a physiological point of view it would be very desirable to ascertain this point, as it would satisfactorily account for the detention of the ovum, and refute the opinion entertained by some physiologists, that the semen must be in actual contact with the ovum for the production of the species.

"From the complete state of obliteration of the tube, I must confess I feel inclined to suppose, that it was not effected by adhesion in consequence of inflammation from the last conception; or else, from the same cause, why was not the extremity of the tube obliterated?"—p. 445.

We have already given our readers the substance of the next paper which we should otherwise notice, intitled, *Microscopic Observations on the Structure of Bone;* by John Howship, Esq. and also, that of a pathological communication, containing *Observations on the Condition of the Bones in Rickets, with an account of some circumstances not before noticed, relating to the Process of Restoration which takes place in them;* by Edward Stanley, Esq.* We have now only to remark, that both essays possess very considerable interest and merit.

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* Vide *Repository*, vol. vii. p. 5, 6.

The next pathological paper is an *Account of a Case of curious Imperfection of Vision;* by Whitlock Nicholl, M.D. The subject of this imperfect vision is a boy eleven years old, healthy and intelligent, with grey eyes, having a yellow tinge surrounding the pupil. The peculiarity consists in his confounding green colours with red, or with brown, according to the degree of the darkness of their shades: thus; pale green he calls light red; common green, red; and dark bottle green, brown. With red also he confounds dark yellows and light browns, although he calls light yellow by its proper name. Blues of every shade he designates properly, but light red and pink he also calls light blue. This peculiarity seems to be derived from the mother's side of the family, her father and one of his brothers having had the same imperfection of vision. No attempt is made to explain the circumstance.

We know an instance of a young lady, who was suddenly attacked, whilst in Jamaica, with a violent pain of the head, which was relieved by bleeding; but for a very considerable time afterwards, every object appeared of a fiery red colour, and this deception continued for some weeks after she returned to England. The eyes, when examined, did not indicate any appearance of local disease, by which the phenomenon could be explained.

The surgical papers are seven in number. The first which we shall notice, the first also of the part, but the twentieth of the volume, consists of *Observations and Cases relating to the Operation for Artificial Pupil;* in a letter from Mr. Maunoir, of Geneva, to Professor Scarpa, of Pavia, with the Professor's answer. Mr. Maunoir's first operation was on the Marquis of Beaumanoir. From a narrative drawn up by the patient himself, it appears that the sight of both eyes had been lost from successive attacks of inflammation; and when Mr. Maunoir examined them, "they were large, prominent, the upper eyelid rather red than white, and slightly swelled; the conjunctiva covered with vessels a little too full; the iris of a bluish grey colour, and presenting, instead of the pupil in the centre, a white spot of the size of a small pin's head."—p. 305.

It was impossible to ascertain correctly the state of the crystalline lens behind this opacity of the capsule, which was adherent to the iris in both eyes; but its transparency was inferred, as the patient "preserved the faculty of distinguishing very clearly the transition from darkness to light." The operation was performed in the following manner:—

"The patient being seated on a chair, and having the head inclined upon a cushion, I placed myself behind him, and with the fore finger of the left hand confining the upper eyelid, whilst an assistant depressed the lower; I made with the right hand a semicircular incision in the lower and external part of the cornea. This incision occupied a full third of the circumference of the membrane. A quick movement of the eye during the operation, obliged me to bring the edge of the instrument so low as to cut slightly the conjunctiva, which occasioned a slight hæmorrhage; but the application of a sponge dipped in cold water and a moment's repose, remedied this little accident. On re-opening the eye, the iris was seen projecting a little from the wound in the cornea. I replaced it with the blunt point of my scissors. Introducing the two blades closed into the anterior chamber, and then opening them, I caused the pointed blade to penetrate the iris, leaving the blunt blade between that membrane and the cornea; then closing the scissors, a perpendicular incision of the iris resulted, describing a little more than half the chord of an arc of two fifths of the circumference of the iris, traced on the side of the temple. This first incision not having occasioned the formation of a pupil of the necessary size, I introduced the scissors into the iris a second time a little obliquely, and immediately the pupil appeared of a satisfactory form and size, but exhibiting the crystalline entirely opake.

"The second stroke of the scissors had divided the capsula: I therefore introduced a small curette, in order to endeavour to destroy what adhered of the crystalline to the shrunk and contracted circumference of the old pupil. It will be seen presently that this attempt did not succeed. Lastly, I effected the passage of a portion of the opake lens by means of a slight pressure with a large scoop exercised on the lower part of the globe of the eye. The crystalline, which was of a cheesy consistence, came out with the greatest ease, and though it was not entirely removed, yet a sufficient quantity was discharged to leave the artificial pupil of a most perfect black. This new pupil was on the side of the temple; and at the exterior and lower part of the iris. The old pupil which had neither changed in form or size, and remained always closed by a white opake body, was not comprised within the area of the new pupil, but was situated internally, and above it. The new pupil had the form of an irregular trapezium.' The Marquis received immediately some confused idea of objects in the light."—p. 307.

The patient could not distinguish any object for the first three weeks after that operation, but before four months had elapsed, the remainder of the lens being absorbed, he could read "written characters of the size of a fourth of an inch," and could otherwise see tolerably well. The other eye was operated on in a similar manner, except that the lens was altogether extracted; but the sight obtained was less perfect. Two other operations are also described, both of which were attended with successful results. In the last of these the crystalline was opake, and adhered to the lower circumference of the iris, and the edge of the closed pupil: but this circumstance, Professor Scarpa, in his answer, regards as of little importance, and recommends the extraction of the lens with its capsule, through an incision "upon the iris proportioned to the size of the body to be extracted." He also advises that, after the complete "extraction of the crystalline with its opake capsule, by means of the least possible introduction of instruments, the artificial pupil ought not to be too near the incision in the cornea." We extract the following suggestions of this illustrious surgeon for the gratification of our readers :—

"After reflecting attentively upon this situation of the artificial pupil, and upon the obstacles which it presents to the operator, it appears to me, if I am not greatly deceived, that a method of operation compounded of that of Wenzel, and of your own, would perfectly answer the desired end. Wenzel, as you are aware, made an incision upon the cornea and the iris with a single stroke of his instrument, taking care that this tranverse incision should pass through or underneath the centre of the enclosed pupil. He then took off, by means of the scissors, a portion of the edge of the iris, for the double purpose of extracting with facility the opake crystalline with its capsule, and of leaving a permanent artificial pupil of sufficient size. In the method which I would suggest, after having made, in the manner of Wenzel, a transverse incision in the iris and in the cornea, I would introduce your scissors, blunted at both points, into the anterior chamber of the aqueous humour, with which I would make an incision in the iris, diverging from the cut made by the knife, so that your usual triangular edge might be the result, having a curvilinear side. This aperture, which requires only a single stroke of the scissors, will be, I think, sufficiently large to allow easy egress to the crystalline and the capsule; and this so much the more easily, in proportion as the point of adherence of the capsule to the iris, is comprehended, either entirely, or in a great measure, within the two incisions. By this means the facility of making the crystalline and the capsule pass obliquely out from the iris will be increased, on account of the enlarged space that will result from the cut with the scissors diverging from that made by the knife; and I should prefer this incision with the knife to the puncture made by you in the iris of Saillard, to afford a passage to the blade of the scissors. Besides this, the direction and the situation of the triangular edge of the iris will be calculated to leave a pupil not only permanent, and sufficiently large, but also placed opposite to the cut in the cornea, and accordingly more convenient for the purposes of vision; especially if it fall upon the side of the iris nearest the nose, which ought, if possible, always to be the case."—p. 318.

The next surgical paper is a *Case of a Wound of the Peroneal Artery, successfully treated by Ligature;* by George James Guthrie, Esq.; and is marked by that judgment and decision, which so strongly characterize every operation of this excellent surgeon. The artery was wounded on the 18th of June, at the battle of Waterloo, by a musket ball, "which entered the right leg immediately behind and below the inner head of the tibia, inclining downwards, and under or before a part of the soleus and gastrocnemius muscles, and coming out through them four inches and three quarters below the head of the fibula, nearly in the middle, but towards the side of the calf of the leg."—p. 332.

No considerable hæmorrhage occurred, however, until the 1st of July, when the tourniquet was applied. Notwithstanding a considerable quantity of blood had been lost, yet there were symptoms of considerable fever: the pulse was 110; and, on passing the finger into the external opening of the wound, a sort of small aneurismal tumour could be felt under the fibula; and when this was pressed against, the hæmorrhage ceased, "indicating that the peroneal artery was in all probability the only vessel wounded." As there was much blood effused between the muscles, which might have in the event of inflammation ensuing, led on to gangrene, the operation was determined upon, and thus performed:—

"The man being laid on his face with the calf of the leg uppermost, I made an incision near seven inches in length in the axis of the limb, taking the shot-hole nearly as a central point, and carried it by successive strokes through the gastrocnemius and soleus muscles towards the peroneal artery, which I attempted to discover, but this was more difficult than might be supposed, after such an opening had been made. The parts were not easily separated, from the inflammation that had taken place, and those in the immediate track of the ball were in the differing stages from sphacelus to a state of health, as the ball in its course had produced its effect upon them, or their powers of life were equal or unequal to the injury sustained.

"The sloughing matter mixed with coagulated blood readily yielded to the back of the knife, but was not easily dissected out. The spot where the arterial blood came from was distinguishable through it, but the artery could not be perceived, the depth of the wound rendering any operation on it difficult. To obviate this inconvenience, I made a transverse incision outwards from the shot-hole to the edge of the fibula, which enabled me to turn back two little flaps, and gave greater facility in the use of my instruments. I could now pass a tenaculum under the spot from whence the blood came, which I raised a little with it, but could not distinctly see the wounded artery in the altered state of parts so as to secure it separately. I therefore passed a small needle, bearing two threads, a sufficient distance above the tenaculum, to induce me to believe it was in sound parts, but including very little in the ligature, when the hæmorrhage ceased: another was passed in the same manner below, and the tenaculum withdrawn. The coagula under the soleus were removed, the cavity washed out by a stream of warm water injected through the external opening, the wound gently drawn together by two or three straps of adhesive plaster, and the limb enveloped in cloths constantly wetted with cold water. The patient was placed on milk diet."—p. 334.

The cure went on favourably; the ligatures came away on the eighth day from the operation; and no untoward circumstance occurred but a small abscess on the edge of the soleus, which soon closed after its contents were evacuated. The man, who, at the time of writing the case, was in the York Hospital, could walk for a short distance without appearing lame. He suffered an occasional cramp in the ball of the foot, but this is attributed rather to some additional injury done to the nerves by the passage of the musket ball, than to the operation.

[To be Continued.]

INTELLIGENCE.

Domestic.

Medical Lectures.

THE lectures of the Medical Institution of Harvard University will commence, at the Medical College in Boston, on the third Wednesday in November.

Anatomy and Surgery, by Dr. **Warren.**
Chemistry, by Dr. **Gorham.**
Principles and Practice of Midwifery, by Dr. **Channing.**
Materia Medica, by Dr. **Bigelow.**
Theory and Practice of Medicine, by Dr. **Jackson.**

Dr. **Jackson** is Dean of the Faculty of Medicine, for the following year.

Medical Graduates in Harvard University.

At the public examination for the degree of Doctor in Medicine, held at Cambridge, in August, the remainder of the Candidates defended their dissertations, and received the degree of Doctor in Medicine at the public commencement.

1. *James Barr*, jun. of New Ipswich, on "Dropsy." 2. *Alfred Baylies* of Taunton, Dissertation on "Pneumonia." 3. *John J. Carlton* of Andover, on "Dysentery." 4. *Frederick Cushing* of South Berwick, on "the causes, symptoms and mode of treatment of the disease called Necrosis." 5. *Ebenezer Hobbs* of Weston, on the internal use of Cantharides. 6. *Samuel Johnson* of Andover, on "Cholera Morbus." 7. *Amos Nourse* of Bolton, on "the Ergot of Rye, employed in Amenorrhæa and Uterine Hæmorrhage." 8. *Seth Washburn* of Northampton, on "the use of Corrosive Sublimate in Typhus gravior."

N.B. The names of Medical Graduates at the Spring examination have already been published.

Prevalent Diseases.

The early part of the summer was remarkable for its uniform warmth, and for the regular descent of moderate showers of rain. These causes produced a season of uncommon fertility. In the month of July, the town of Charlestown, separated from Boston only by Charles river, was assailed by a malignant dysentery. Soon after, the same disease appeared in Cambridge, at about three miles from Charlestown, and with similar symptoms. The soil of Cambridge is sandy and dry. The place is remarkable for its healthiness. That part of it, which was the principal seat of the disorder is low, and near the river, in which the tide flows, and near the salt marshes, which border on the river. In one of the small streets thus situated, almost every family was affected, and in one family, four persons became victims of the disorder. The invasion of disease was preceded by the appearance of a change in the secretions of the tongue; by nausea, by loss of strength, and pain, not generally very acute, in the lower part of the abdomen. Then followed evacuations from the bowels, once in 30, 15 and in some cases, once in 10 minutes, accompanied with pain and tenesmus. The pulse very quick, often weak. The skin not so hot as in common dysentery, nor the febrile paroxysms so long, nor so severe. But, however violent the appearances of fever were at first, they soon diminished, the skin inclined to coldness at least in the extremities, the pulse became extremely quick and weak, and the tongue black and dry. A few persons were struck down at once, and without any distinct paroxysms of fever, and even without frequent discharges from the bowels, died in three or four days. A greater number died between the tenth and fifteenth. Those who reached the fourth week of the disease, presented a clean, deeply red tongue, frequent discharges of dark coloured blood, or small quantities of mucus, and great debility. The tongue soon became apthous, and the patient slowly wasted away. Emetics sometimes produced a temporary remission of the disease. All the purgatives seemed to be used without benefit. Injections of ipecacuanha, of opium, or of mucilaginous substances were not usually productive of any advantage. If any particular plan of treatment had influence on the disease, it seemed to be that in which tonics were combined with purgatives, at an early period, the tonics being afterwards pursued very closely, until a remission of the symptoms took place.

The disease at Cambridge, prevailed in the months of August and September, and proved fatal to about fifty people; double the number of deaths which usually occur in that place in a year. Soon after the appearance of this disorder at Cambridge, complaints of the stomach and bowels become prevalent in Boston. At first, a great number of cases of cholera morbus were observed, with symptoms unusually violent. The vomiting commonly attendant on this complaint was so obstinate, that often a number of days passed without the possibility of procuring evacuations from the bowels; but when it did happen that evacuations were produced, the patients were relieved, and rarely fell into dysentery or fever. It may be remarked that the appearance of bile was not uniformly attendant on the vomiting in these cases: but that, on the contrary, in some of the worst specimens of the disease, no bile was exhibited, either from the stomach or intestines, from the commencement to the close of the disease. In such cases, it is fair to believe that this cholera morbus, so called, was not brought on by excess of bile in the stomach or intestines; but rather by want of that liquid, and the existence of indigested, or morbific substances: by morbific it is meant to say putrid, acid or acrid matters. In the treatment of cholera morbus, three methods of attacking the disease were employed, according to circumstances, 1. By emetics. 2. By purgatives. 3. By opium. On the whole it appeared that those cases, where emetics could be administered and were administered in the beginning, terminated most speedily and favourably. Bleeding, so far as it was employed by the reporter, did not mitigate the pain, nor check the vomiting.

The dysentery was marked by a want of those symptoms of high inflammation, which used to accompany it. Hence bleeding could rarely be employed with advantage. The evacuations were commonly of blood or bloody mucus, and excessively frequent. The tongue, at first covered with a whitish furr, gradually changed this appearance for a brown or black colour, with a disposition to become dry. The pain was not so atrocious as in the more inflammatory dysenteries, but the tenesmus was a very constant and troublesome symptom, and soon produced such swelling of the rectum, as to prevent the use of injections. The severe cases lasted two or three weeks; and even after this time, the appearance of blood in the evacuations, continued for some time. In the treatment of this disorder, small emetics, frequently repeated, had a sensible effect in alleviating the symptoms. The use of these remedies was followed or alternated with purgatives, among which there seemed to be none, decidedly preferable. Small purgative doses of jalap with the submuriate of quicksilver, senna with aromatics, and the saline purgatives were mostly employed. Few patients could dispense with the use of opium at night, and many had it continually, in combination with the purgatives. The submuriate of quicksilver could not be made to produce its peculiar effect on the system, and of course was of no use, but as a purgative. Injections, even with opium, were not useful; and those of ipecacuanha were injurious. The injection of warm olive oil gave relief in a few instances. After the disorder was a little advanced, the symptoms demanded the use of bark in combination with the purgative medicines; and also a moderate use of wine. Rhubarb had an excellent effect in the late stage of the disease, but never operated favourably in the beginning. Of about a hundred cases of cholera morbus, diarrhaea and dysentery, only two proved fatal. Of these two, one was a teething child, and the other an adult, in whom the symptoms of typhus mitior were blended with those of dysentery.

The disorder, known by the name of typhus, or typhus mitior, or slow nervous fever, has also been more prevalent than common, though rarely fatal. Many families have been affected by it, in a succession of individuals, in such a manner as to lead to the suspicion of this disease being contagious; but the most remarkable evidence of contagion is now existing in the Boston Almshouse, where, from a few individuals in the sick wards, nearly all the inhabitants of those wards were suddenly attacked, and soon after, the inhabitants of the neighbouring wards. The concentration of disease, from the great number of subjects, produced considerable mortality in this house under every variety of treatment. The bodies examined presented marks of general inflammation. The mucous coat of the stomach, small and large intestines, and the serous coat of the brain were notably inflamed. In the treatment of this fever, it has been noticed, that the constitutional affection by mercury did not as usual cure the disease; so that many practitioners were led to abandon this treatment altogether. The most successful plan appeared to consist in the judicious administration of emetic and purgative medicines; though it must be confessed that the antimonial practice has considerably revived among us, whether with advantage is not, probably, settled.

The remitting fever of children has been rather common.

The frosts of October have cut short the principal part of these diseases; and in their place catarrhal complaints appear, though not with severity, except in some infants.

Note. These remarks are to be understood, as proceeding from an individual.

Foreign.

On the Author of the Theory concerning the Secretion of Pus; by James Curry, M.D.

FOR your being the first in communicating to the British faculty the ingenious and candid critique, by the New-England Journalists, upon Caldwell's Notes, &c. on Cullen's Frst Lines, accept my grateful acknowledgment. I equally approve of your candour respecting the disputed claim to the Glandular theory of Suppuration, and trust that the opinion which I have taught in the medical school of Guy's Hospital for sixteen years past, will, when explained, not be found to call for more than the "regret" you express "at hearing of such doctrines being promulgated in one of the London schools" (Med. and Phys. Journal, No. 222, for July, 1817, note,* on p. 163,) viz. that Dr. Morgan of Philadelphia, and not Mr. John Hunter, was the first who affirmed that *pus was a secretion.* Now, sir, I am proud thus publicly to avow, that, in the doctrines I have taught, and, God willing, shall ever teach, I have never been guided by any other motive than a sincere belief of their being well founded; and that, although not devoid of national partiality, where that is justifiable, I have never sacrificed truth to national attachment. On this ground alone I have always followed the precept of *detur digniori,* and awarded praise where praise was justly due, without a moment's consideration of the claimant's era, or country: "*Tros Tyriusve mihi nullo discrimine agetur.*" Although, perhaps, *in a practical view,* the question respecting the formation of pus is of little consequence, yet I have always been impressed with a conviction, that what many hold to be mere speculative questions in science, are, nevertheless, worthy of notice, upon this principle, that, in natural knowledge, every fact has a bearing, however remote, upon some other and more important ones, which, perhaps, yet remain to be discovered, but which *this seemingly insulated one* will serve to illustrate and confirm. Thinking so, I could not pass over unnoticed a point which had been successively laboured on by Boerhaave, Grashius, De Haen, Quesnay, Pringle, Morgan, and John Hunter; and, allowing any merit to the *glandular theory,* I could not avoid giving that merit to Dr. Morgan, who discussed the question with great ingenuity in his Inaugural Dissertation, on taking his degree at Edinburgh in 1763; whilst I could find no proof that Mr. Hunter had taught, or even adopted, such opinion, until a considerably later period: for Sir Everard Home, who, in 1788, published a "Dissertation on the Properties of Pus," and claims the merit of the doctrine as entirely belonging to Mr. Hunter, has altogether forgotten to support his claim, by stating at what time it was first formed and promulgated by him. It is, indeed, very possible, that Sir Everard did not think such a step at all necessary; for he never once notices Dr. Morgan's Thesis, and might not even know that any such thing existed. It is often said, and, in certain things, I think it true, that we grow fond of early opinions, and more tenacious of them as we become older; but I shall feel sincere gratification in being set right, if I am wrong, upon this point, and shall not fail to make the *amende honorable* to the memory of John Hunter, by transferring from Dr. Morgan to our British Machaon the exclusive claim to originality. I may here remark, by the way, that the first *hint* of this doctrine will be found in Dr. Simson's "Tractatus de Re Medica," published in 1740, in which he asks, "may not an issue be considered as performing the office of a gland?" and I think it not improbable but that Dr. Morgan took this *hint*, as Simson's book, though now little valued or read, was then common in the hands of students at Edinburgh.

After having said so much upon this question, it may seem odd that I should wind up by stating that I believe the doctrine itself *untrue*, nay more, that it is even completely overturned by the very experiments which have been adduced in its favour. The detail of arguments, however, to prove what I here advance, would take up much more time than I can at present allot to it; but, should the question be thought worthy of farther discussion, I pledge myself to make good the conclusion I have drawn.

Bridge-street, Blackfriars;
August 1, 1817.

We shall feel extremely thankful to Dr. Curry by the honour of a further communication on this interesting subject; and we trust Mr. Hunter's friends will not be backward in defending their master.—EDIT. *Lond. Med. and Phys. Joun.*

Test of Arsenic.—Dr. Paris has proposed the following method of employing the *nitrate of silver* as a test of the presence of arsenic. "Instead of conducting the trial in glasses, drop the suspected liquor upon writing paper, making a broad line with it. Along this line, if a stick of lunar caustic be slowly drawn, a streak is produced of a colour resembling that called *Indian yellow;* and this is the case whether arsenic or a phosphoric salt be present; but the one from arsenic is rough and curdy, as if effected by a crayon; the other, quite smooth and even in its appearance, such as would be produced by a water colour. A more important, and still more unequivocal, mark of distinction soon succeeds: in less than two minutes the phosphoric yellow fades into a *sad green,* becoming gradually darker, until it becomes black: the arsenical yellow, on the other hand, remains permanent for some time, when it becomes brown. These experiments should not be made in the sunshine, as the transition of colour is then too rapid.—*Annals of Phil.* vol. x. p. 60.

Mr. A. T. Thomson, one of the Editors, in repeating these experiments, found, that if the above directions be strictly adhered to, the test is ambiguous; but if modified in the following manner, it is one of the best for detecting the presence of arsenic hitherto suggested. When the lunar caustic instantly produces a very bright yellow, we may suspect it is occasioned by a phosphoric salt; and this is rendered certain, if, notwithstanding, it be brushed over with liquid ammonia, the colour becomes in a few minutes a sad green, and, as it dries, a dark claret brown. If the suspected solution contain arsenic, combined with potash, as in Fowler's solution, or that of the London Pharmacopœia, the streak will be curdy, as Dr. Paris describes; but of a very pale yellow colour. If it contain alkali, it will be indistinct, and of a greenish yellow tint; and if any muriate be present, it will be nearly white. The presence of arsenic, however, is unequivocally determined in all, if, on brushing them over with liquid ammonia, a bright queen's yellow be instantly produced. This remains permanent for nearly an hour; and, in changing, the first and second acquire a light or yellowish mahogony brown colour, and the third a very dark brown; but after some hours, the whole becomes nearly black.

Antidote of Prussic Acid.—*M. Virey,* from observing the effects of sulphate of iron in restoring a cow, who was nearly killed by the essential oil of bitter almonds, proposes the solution of sulphate of iron as the antidote of prussic acid.

Tic Doloureux.—A correspondent informs us, that he has seen a tincture of the leaves of Atropa Belladonna, made in the same manner as the tincture of henbane of the London

Pharmacopœia, advantageously employed as an external application in two cases of tic doloureux.

Cure of Epilepsy.—A young man, who had been epileptic for eight years, and who had tried various remedies, was at length cured by taking the superacetate of lead, in doses of three grains, night and morning for five days. The use of it was begun three days before the moon was at the full, and then repeated for five lunar periods. After each dose a table spoonful of olive oil was swallowed. M. Eberle, who details the case, observes that this remedy is more likely to succeed in cases in which the accessions are regular and the patient strong.—*Gazette de Santé.*

Aneurism.—Signor Palleta, in operating upon a popliteal aneurism, according to the method of Scarpa, having removed the ligature on the fourth day, the wound healed by the first intention, and the cure was perfected on the fourteenth day after the operation.—*Giorn. di Fisica, 2d Bis.* 1817.

Artificial Congelation of Water.—Professor Leslie, in a letter to Dr. Thomson, has announced, that parched oatmeal has a stronger power of absorbing humidity than even decayed trap, and hence may be advantageously used for freezing water in vacuo. He states, that with a body of dried oatmeal a foot in diameter, and rather more than an inch deep, he froze a pound and a quarter of water contained in a hemispherical porous cup, although the temperament of the room was above 50°. The Professor adds, " that when the experiment was repeated, and the surface of the water about double that of the meal, this substance acquired, after the air under the receiver had been rarified, a heat exceeding 50° of Faht., so as to feel indeed sensibly hot on applying the hand."—*Annals of Phil.* vol. x. p. 61.

Volatility of Mercury.—Dr. Hermbstaed has ascertained that the minimum of the temperature at which mercury is volatilized is 80° Farenheit: hence it is easy to explain why workmen employed in the silvering of looking glasses are sometimes salivated.—*Giornale di Fisica, &c. 2d Bisnestre,* 1817.

Colouring Matter of the Blood.—Berzelius has lately examined the opinion deduced by Mr. Brande from his experiments upon blood, and confirmed by those of Vauquelin, that the colouring matter of the blood is perfectly free from iron. He has been led to an opposite conclusion, from having found that the ashes produced by incinerating this colouring matter are one half oxide of iron; and conceives he is authorized in regarding his former opinion on this subject confirmed—(vide *Animal Chemistry.*) He asserts, therefore, that, as the colouring matter of the blood is distinguished from colourless animal substances by the quantity of oxide of iron which it yields when calcined, there is some reason for thinking that iron contributes to give blood its deep colour.—*Annales de Chim.* t. v. p. 42.

Lactometer.—An instrument for the purpose of ascertaining the relative quantity of cream produced from milk, has lately been constructed by Mr. Jones, mathematical instrument maker, Charing Cross. It consists of several glass tubes resembling test tubes mounted on a stand, each about eleven inches long and three quarters of an inch in diameter, closed at one end, and open and a little flanched at the other. At ten inches from the bottom of each tube, a mark is made upon the glass, having an o (zero) placed against it; and, from this point, the tube is divided into tenths of inches, and graduated downwards for three inches, so that each division is 1-100 of the tube. By filling these tubes with the new milk of different cows, the percentage of cream will be evident upon inspection.—*Journal of Science and the Arts.*

Upon a recent ballot for the place of Foreign Associate of the French Royal Academy of Sciences, in the room of M. Klaproth, deceased, M. Scarpa was elected; 27 votes having combined in his favour, and 21 only for Sir Humphry Davy.

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